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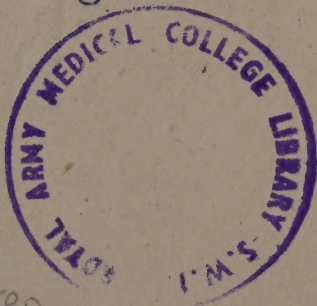
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RESULTS OF AN INVESTIGATION,
RESPECTING
EPIDEMIC AND PESTILENTIAL
DISEASES.

The all-surrounding heav'n, the vital air,
Is big with death; and tho' the putrid south
Be shut, tho' no convulsive agony
Shake from the deep foundations of the world,
Th' imprison'd plagues, a secret venom oft
Corrupts the air, the water, and the land.
What livid deaths has sad Byzantium seen!
How oft has Cairo, with a mother's woe,
Wept o'er her slaughter'd sons, and lonely streets!
Ev'n Albion, girt with less malignant skies,
Albion the poison of the Gods has drank,
And felt the sting of monsters all her own.

ART OF PRESERVING HEALTH, B. III. L. 521.

RESULTS OF AN INVESTIGATION,

RESPECTING

EPIDEMIC AND PESTILENTIAL

DISEASES;

INCLUDING

RESEARCHES IN THE LEVANT,

CONCERNING

The Plague.

BY

CHARLES MACLEAN, M.D.

LECTURER ON THE DISEASES OF HOT CLIMATES TO THE
HONOURABLE EAST INDIA COMPANY.

IN TWO VOLUMES.

VOL. II.

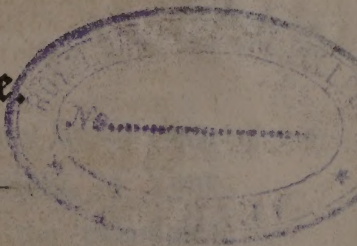
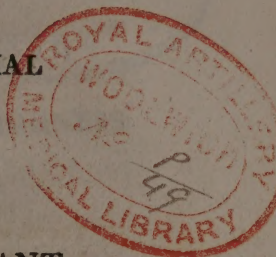
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1818.



RESULTS OF AN INVESTIGATION

REGISTERED

EPIDEMIO AND HYGIENE

THE DISEASES OF

RESEARCHES IN THE

THE DISEASES OF

in the

THE DISEASES OF

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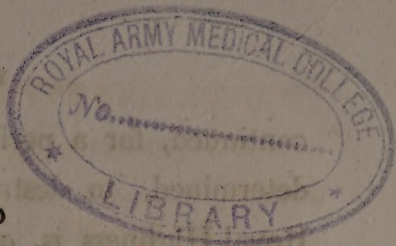
THE DISEASES OF

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THE DISEASES OF



TO

FIELD MARSHAL, HIS ROYAL HIGHNESS
THE DUKE OF KENT,

&c. &c. &c.

SIR,

THE fruits of my Researches, in the Levant, belong, of right, to Your Royal Highness. When the principles promulgated in this work, shall have been finally established, the world will naturally desire to know, to whom they are indebted, for the benefits, which will result from their application.

But for the discernment, and true philanthropy of Your Royal Highness, my Investigation of the Plague would never have been accomplished : and the stupendous errors which are here refuted, would have

continued, for a period, which cannot be determined, to destroy mankind. Your Royal Highness is, consequently, as much the cause of the discoveries, which it has produced, as pure air is of health, or its absence of epidemic diseases.

By the application, to practice, of the Results of these Researches, *several millions* of human beings might be *annually* rescued from destruction. That other illustrious personages may be disposed to emulate the enlightened example of Your Royal Highness; and to unite with you, in causing measures to be adopted, toward accomplishing so great, and so benevolent an object, is the ardent prayer of

Your Royal Highness's

Dutiful and devoted Servant,

CHARLES MACLEAN.

LONDON,
10th April, 1818.

P R E F A C E

TO

VOL. II.

THIS Work consists of two distinct branches: the one composed of refutations of some of the most ancient and stupendous errors, that have concurred, to disgrace medicine, and to destroy mankind: the other, of elucidations, in a manner wholly original, of the nature and cure, and of the cause and prevention, of epidemic, and pestilential maladies. Both are investigated according to the principles of inductive philosophy.

In the first volume, it has been shewn, by conclusions, logically deduced, from un-

deniable premises, that it is impossible epidemic diseases should ever depend upon contagion :

I have so proved it,
That the probation bears no hinge, nor loop
To hang a doubt on.

The fraudulent origin of this pernicious error has even been distinctly traced to its source: and the destructive consequences, to mankind, of the institutions, which have arisen from it, have been established, by irrefragable demonstration.

From the numerous public interests, which this momentous enquiry involves, it imperiously demands the interposition of governments. And, by the facts, which I have collected and arranged, it is happily placed within the power of every person of liberal education, without the aid of technical knowledge, to appreciate the value of my doctrines.

But, respecting the nature and cure, and the cause and prevention, the case is materially different. Those conclusions, which have been deduced from experiments proper to myself, can only be confirmed, or refuted, by the repetition of similar experiments; and must be left to be appreciated by time and experience. In no case, can they be submitted to the dictates of authority. For, if every physician in the universe should think fit to maintain, that the air, in its quality of an exciting power, is not the cause of epidemic diseases; that blood-letting is a cure for every malady; and that mercury never cures fevers, but in consequence of its evacuant operations; whilst the results, upon the proper experiments, under unequivocal circumstances, being made, were to prove of a diametrically opposite nature, what philosophic enquirer, rejecting the mode of induction, would think himself justified in giving credit to the evidence of

tradition and testimony? Yet such has hitherto been the foundation of almost all the principal conclusions in medicine!

Thus, in the very nature of the evidence, which has been heretofore almost universally resorted to, in medical disquisitions, we find one of the most extensive sources of mortality and delusion. It is not, however, that the evidence applicable to medicine, is, in its nature, uncertain; but, that what has been usually adduced as evidence, has been of an improper kind! The modes of proof, or the materials of conviction, which are alone appropriate, in this department, I have endeavoured so to define, that medical researches may, in future, be conducted with more certainty and decision.

Amongst the most pernicious of the numerous errors, which, upon the mere ground of hypotheses, have pervaded medicine, al-

most from the origin of societies, is the deleterious practice of blood-letting. This species of evacuation, although injurious in all diseases, is, in its consequences, pre-eminently destructive, in those, which are epidemic and pestilential. That blood-letting is an operation, which ought never, under any circumstances, or in any situation, to be performed, I have shewn, in this volume, by a great variety of proofs. My observations, although hitherto never published, as a whole, were almost all written, a great many years ago, chiefly in the East Indies.

In the Narrative of my Researches, in the Levant, concerning the Plague, which occupies a considerable portion of this volume, I have chosen partly to retain the form of a diary; conceiving, that, incidents related in the order of their occurrence, may often serve, better than systematic reasoning, to display the advantages of truth, or the injurious consequences of error.

I have also enriched this volume, by some account of the experiments of Drs. Whyte, and Valli, and Mr. Von Rosenfeldt.

Some extraordinary attempts having been made, since the publication of my first volume, by confounding opinion with proof, hypothesis with theory, and authority with induction, to identify the unfounded pretensions of other persons, with my discoveries, it is due to myself, without descending to notice individual pilferers, to enter my protest generally, against such flagrant acts, of literary, and scientific depredation.

LONDON,
10th April, 1818.

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PART III.

RESEARCHES, IN THE LEVANT, CONCERNING THE NATURE AND CAUSE OF PLAGUE.

“ At last a knowing age began t' enquire.”

REL. LAICI.

FROM its first promulgation in 1546, to the year 1796, being a period of two hundred and fifty years, I am not aware, that any attempt had been made to refute the doctrine of contagion, generally, as the cause of epidemic and pestilential diseases. The first, and, there is reason to believe, the only refutation of that doctrine, which has yet appeared, is contained in a Dissertation, published by myself, that year, in India. It constitutes one of the earliest specimens of the application of the principles of inductive philosophy, at least to this department of medicine. My proofs consisted of conclusions logically deduced from

undeniable premises. But inferences in the abstract were not capable of shaking the inveterate prejudices, which had been spreading, and taking root, for two centuries and a half, throughout Christendom. I felt, that, on this subject, the reputation of practical experience, in the highest species of pestilence, the plague of the Levant, would be indispensably necessary, to command assent, even to the most irrefragable conclusions. But, owing to the peculiar circumstances of the times, notwithstanding my most assiduous endeavours, it was nineteen years, before I could obtain such practical proof, by direct experiment, of the validity of the principles, which I had previously promulgated, respecting epidemic diseases, as to be enabled, with an irresistible accumulation of evidence, and the certainty of producing general conviction, to offer them, in a new shape, to the world.

In their present shape, I trust I may be allowed, without the imputation of exaggeration, or inordinate vanity, to apply to the results of my investigation, respecting epidemic diseases, the words of a noble Poet :

“Such secrets are not easily found out,
But once discovered leave no room to doubt.
Truth stamps conviction on the ravished breast.”

ROSCOMMON.

BOOK VI.

RESEARCHES AT MALTA.

“—— Mute the voice of joy,
And hushed the clamour of the busy world :
Empty the streets, with uncouth verdure clad :
Into the worst of deserts sudden turned
The chearful haunt of men ; unless escaped
From the doomed house where matchless horror reigns,
Shut up by barbarous fear, the smitten wretch,
With frenzy wild, breaks loose, and, loud to heaven
Screaming, the dreadful policy arraigns,
Inhuman, and unwise.”

THOMSON.

CHAP. XX.

Departure from England, and arrival at Malta—enquiries there—facilities afforded to my researches by Lieutenant-General Sir Thomas Maitland, and the Officers of Government.—Circumstances, and mortality, of the pestilence, of 1813—its commencement, and cessation take place at the usual season, in similar latitudes—its decline erroneously attributed to the wisdom of the measures of a new Council of Health, and of a new Plague Police establishment ; and its cessation to the energy of the proceedings of the new Governor.

ACCREDITED by the British government, and the English Levant Company, for the purpose of investigating the plague of the Levant, I em-

barked at Gravesend, on the 29th of May, 1815, on board the ship *Tiger*, William Llewellyn, Master, bound to Constantinople.

After an unfavourable passage down the channel, and some days detention at Falmouth (which by the bye, a foreign traveller, by the help of a fertile imagination, has found to bear a strong resemblance to the capital of Turkey) we proceeded on our voyage, without meeting with any remarkable occurrence, and arrived on Saturday, the 24th of June, at Malta.

Early the following morning, I repaired, accompanied by Mr. Shoolbred, the British Chancellor at Constantinople, to pay my respects to the Governor of the island, Lieutenant-General Sir Thomas Maitland. Having announced to his Excellency the object of my voyage, he was pleased to express himself in strong, and rather discouraging terms, respecting what he conceived the hopeless nature of my enterprise; but frankly and cordially offered me his assistance toward the prosecution of my investigation. Being himself about to embark, in a few days, for Sicily and Leghorn, he appointed me to attend him at the Government House, at six o'clock the succeeding morning, in order to inspect the quarantine station, lazaretto, &c. At setting out, we were joined by Robert Grieves, Esq. Deputy Inspector of Hospitals, and Chief of the Medi-

cal Staff, to whose friendly attentions I was so much indebted, during my subsequent residence at Valletta. After a minute inspection of several hours, I remained fully persuaded, that, were such institutions capable of fulfilling the object for which they were intended, those of Malta would have deserved to be considered as models for imitation. The capaciousness and arrangements of the quarantine harbour, divided by the whole extent of the city from that which is in ordinary use; the cleanness, state of repair, and convenience of the buildings; the divisions, inclosures, and improvements recently made, and making, on the face of the island, on which these buildings are situated, and which lays nearly in the center of the quarantine harbour; together with the complete organization of the whole establishment, are such as could not fail to have gratified the mind of the benevolent Howard. The accommodations for the performance of quarantine, whether for ships, goods, persons, or cattle^{*}, appeared to approach the highest point of convenience, of which they are susceptible. But, under the view of the subject, which I entertain, my feelings could only be those of regret, that such immense sums

^{*} The cattle imported into Malta, from Barbary, are frequently put in quarantine.

should be wasted in the improvement of establishments, which I deem not simply useless, but injurious, in an almost incredible degree, to many of the best interests of mankind. There is one part of these institutions, however, that, even under the ideas which I hold to be correct, respecting the cause and prevention of epidemic diseases, might, with advantage, be preserved. Although the high walls, erected on the island, should no longer be used, for purposes of restraint; the fields, which they now serve to divide, as the situation appears to be healthy, might be usefully employed in receiving, and accommodating, under tents, such of the inhabitants of Valletta, as might wish to be removed, whenever the air of that city should become sufficiently noxious, to threaten, or to produce a pestilence.

Having returned to the Government House to breakfast, we found the titular Archbishop of Rhodes, the superior clergy, military, and principal inhabitants, assembled, to pay their respects to his Excellency, on the eve of his departure from the island. General Maitland, notwithstanding, continued to honour me with his conversation, for some considerable time longer, and to give orders to the different officers of government, that I might be furnished, in his absence, with all the documents and information,

which I might require, upon the subject of the plague; intentions, in which he was most cordially seconded by his successor, pro tempore, in the government, Major-General Layard; by Alexander Wood, Esq. public secretary; Robert Grieves, Esq. chief of the medical staff, and superintendant of quarantine, at whose house I resided; and other official and private persons. I must also express my acknowledgments to the Abbé Bellanti, librarian, for the great trouble and pains he took, in facilitating my access to the information, contained in the magnificent library of the ci-devant knights of St. John of Jerusalem; an inexhaustible mine, which I had to regret, that the shortness of my stay did not allow me more minutely to explore.

These arrangements being terminated, we joined the gentlemen assembled, to whom, especially those, who had most distinguished themselves, either as members of the Council of Health, or of the Plague Police, during the late pestilence, the Governor was pleased to introduce me, adding to the name of each person some appropriate remarks connected with his merits, upon that trying occasion. Amongst the most noticeable, for their sprightliness and intelligence, were the Vicar-General, and Col. Rivaroli, of De Rolle's regiment, whom he jocosely designated his two principal plague doctors.

Aware, as I was, of the strength and universality of the existing prejudices, respecting contagion, I forbore, in my conversation, any strong expression of my opinions upon that head, farther than common candour seemed to require; endeavouring that my chief expectation of benefit from the proposed investigation, should appear to rest upon my possessing the knowledge of a method of cure, which would probably be found efficient. I could perceive, that, even the slightest expression of doubt, respecting contagion, excited, upon all hands, surmises not the most favourable to the sanity of my intellects; whilst, amongst the clergy, they appeared to give rise to mixed sensations of commiseration and horror, which I was not then able fully to account for, although they now admit of an easy explanation. Those, who peruse the historical facts, which I have since developed, respecting the papal origin of this error, as published in the first volume of these results, will immediately perceive, that, in the eyes of the Catholic clergy of the Levant, it was very natural, that I should have rendered myself, by such doubts, obnoxious to a charge of heresy.

The following are the principal results of my enquiries upon this occasion.

According to a diary, with which I was favoured by Robert Corner, Esq. the first rumour

of plague arose at Valletta, on the 5th of May, 1813, which, on the 6th, was confirmed. It would be superfluous to describe the sources to which the supposed contagion of this malady was attributed in Malta; the doctrine itself having been fully disproved in the preceding volume. I may merely observe, that it was most generally imputed, as usual, to importation by the last vessel that happened to land goods from a suspected quarter; and that many were of opinion the vessel in question ought to have been burnt¹.

It was noted as a remarkable occurrence, that, on the 14th of March, "the light showers that fell in some parts of the island, brought down a reddish earth with them." And, on the 21st of March, Mr. Corner makes this memorandum: "I was this day informed by the officers of his Majesty's ship Sparrow Hawk, that, at Palermo, on the 14th inst. the weather had the same heavy, dull, foggy, appearance, which was remarked here, and that a quantity of muddy earthy substance fell with the rain, but in greater quantity than at Malta."

From documents, which are of unquestionable authenticity, I am enabled to state, that, in the month of May, in the city of Valletta, 51 per-

¹ The Proto-Médico, or President of the College of Physicians, in his 4th Answer, gives it an Egyptian origin.

sons died, and 90 were taken ill; in June 192 died, and 358 were taken ill; in July 315 died, and 471 were taken ill; in August the deaths were reduced to 94, and the number of those attacked with the malady to 211; in September there died but 11, and only 31 were seized; and, in October, the disease might be considered as having ceased, although some straggling cases continued to occur till December.

At Casal Zebberg (the village of Zebberg) the sickness and mortality were proportionably more considerable, and the period at which the evil arrived at its height was somewhat later. In July 144 died, and 146 were taken ill; in August 271 died, and 211 were taken ill; in September 232 died, and 169 were taken ill; in October 9 died, and 10 were taken ill; and from this period the disease decreased rapidly.

In Birchircara, and some other casals, as well as in the neighbouring island of Gozo, the violence and decadence of the disease also generally happened at periods somewhat later, than in Valletta. The succession, in which the inhabitants of adjacent places are usually attacked with epidemic diseases, has been exemplified, in a conspicuous manner, in the plague of London in 1665, and that of Marseilles in 1720: and explained in a manner that will equally apply to

the phænomena of the pestilence under consideration.

The decrease of the disease, in Valletta, in August and September, and in Zebberg, and the other villages, in September and October, have, in conformity with the prevailing opinion respecting its cause, been attributed to the efforts of a new Council of Health, and the greater efficiency of a new Police Establishment, which were instituted at the height of the malady; and its total cessation, to the energy of the measures adopted by Lieutenant-General Sir Thomas Maitland, upon his arrival in October.

But, whilst I most freely admit, that the precautionary measures employed by the respectable gentlemen composing the Council of Health¹, and the new Police Establishment, were such as

¹ The Council consisted of,

The King's Civil Commissioner, *President*.

Major-General Layard, *Vice-President*.

Members.

The Public Secretary,	The Treasurer of the Go-
Dr. G. N. Zammit,	vernment,
Col. Phillips, D. Q. M. G.	The Vicar-General,
Marq. Jer. Delicata,	Lt.-Col. Anderson, D. A. G.
Acting Superintendant of	R. Green, Esq. D. I. Hosp.
Quarantine,	J. Locker, Esq.
The Proto-Medico,	Dr. F. L. Gravagna,
James Chabot, Esq.	The Rev. J. C. Miller, Sec.

necessarily emanated from the opinion, which has universally prevailed, for the last 270 years, in all Christian states, and were, according to that opinion, some of the most fit and proper, which could have been devised; as well as, that the zeal, energy, and talent, displayed by the new Governor, were eminently conspicuous, both in the temporary measures, which he caused to be adopted, upon his arrival, and the more permanent plans, which he afterwards instituted for the improvement of the quarantine, and Lazaretto establishments; I cannot but presume, that, it must be obvious to those, who have perused the proofs, which have been already adduced upon the subject, that the connection of these circumstances must have been mere accidental coincidences; especially when they find, that, in 1813, an epidemic disease, similar to that which afflicted Malta, commenced, spread, declined, and ceased, at periods precisely similar, as is shewn in a subsequent part of this volume, in the Turkish province of Wallachia, where there are neither quarantine, nor plague police.

The measures adopted, upon this occasion, in proportion as they would have been efficient, had the prevailing view of the subject been correct, were, the contrary being true, in the same proportion, vexatious and mischievous. The universal consternation, occasioned by these events, at

Malta; the flight of such of the inhabitants, whose situation would admit of their removing; the ruin of commerce, and the numerous other evils, which had their source in the existing belief, are too recent and notorious to require any particular animadversion. Such, especially in commercial communities, are the invariable consequences of this baleful delusion,

“Till desolation o’er the grass-grown streets
Expands his raven wings.”—

The misery of separation, seclusion, and restriction, was not here confined to the actual period of pestilence. Every straggling case of disease, which afterwards occurred, under circumstances considered suspicious, occasioned a renewal of precautions: and, if the suspicions were thought to be confirmed, a renovation of the most rigid restrictions. Thus, in the month of December, an individual case of suspected sickness, but which proved not to be plague, gave rise to the following Proclamation.

“A case of suspicion having yesterday occurred within the town of Valletta, his Excellency the Governor deemed it prudent to take every precaution upon the occasion;—and though he is happy in stating that the concurrent opinion of all the medical advisers of Government is strongly in favor of its being no case of plague

or of pestilential contagion, he still deems it advisable, as a further precaution, to direct that the opening of the communication generally between Valletta and Floriana, as ordered in the Proclamation of the 4th inst. be postponed until Tuesday next. And his Excellency is further pleased to direct, that, should no new case of suspicion occur, the free communication shall be opened between the whole of the towns on either side of the port, in respect to each other, and, the shipping in the harbour, from the first of the new year.

“ Although his Excellency is strongly prepossessed with the idea that no occurrence will lead him to withhold the granting of general pratique at the end of the ensuing twenty days;—and although he trusts that, under the favor of Divine Providence, no new case will occur after pratique is granted, still, looking at the experience acquired from the history of plague in other countries, he would be grossly deceiving the people of these islands, and no less negligent of his own duty, were he not to state, that, for many months to come, a considerable risk of the recurrence of the late fatal calamity is to be apprehended, and provided against with the most anxious and unremitting solicitude on the part of Government.

“ In this view of the subject, it has been his

Excellency's duty to make such arrangements as to him seem necessary upon the occasion, and he flatters himself, although no human prudence can totally avert the chance of individual cases occurring, that they are of a nature and character to arrest the evil on its first appearance, so as to obviate the danger of any contagion spreading.

“ It is only by promptness, decision, and celerity of action that this great object can be attained; and as nothing is more calculated to enable the Government to act with celerity, than that the governed should be aware of what is expected of them, and fully apprised of the general line of proceeding upon all such occasions, his Excellency deems it expedient to publish the following, as the rules which will be adopted, until the fall of next year, in Valletta, and, as nearly as may be, in every other part of the island, whenever any case of contagion, or reported suspicion may take place.

“ Upon any such occurrence, proclamation thereof will be made by the police officers, attended by a trumpeter or bugles, in the various streets, in order that all persons may retire immediately within their respective houses,—and patrols will be made by the military, for the duly enforcing such Proclamation.

“ On the case being thoroughly investigated and traced to the utmost extent, *the district in*

which it may have occurred being shut in, and the parties, if necessary, conveyed to the Lazaretto, free communication in all the other districts (or such as the nature of the case may admit) will be allowed.

“ Proper officers are named to superintend the various districts, now formed into twenty-six. A regular establishment will be maintained ; and the detailed regulations, specifying all the officers, with their respective duties, as connected with the health of the town, will be forthwith published for general information.”

“ Palace, Valletta, 24th Dec. 1813.”

Considering how little credit can be due to the testimony of prejudiced persons, on such a subject as that of contagion, and the example of the delusive evidence given to Howard, by the persons connected with Lazarettos, or Plague Establishments, in the Levant, I hesitated whether I should at all attempt to avail myself of such doubtful, or impure sources of information. But, aware of the superior importance which is usually attributed to intelligence, however deficient, that appears to be derived from official sources, and feeling that it would be possible to counteract, by the force of logical conclusions, the attempts which might be made, by interested, or ignorant persons, to mislead, or to deceive, I

determined to avail myself of the opportunity, which occurred at Malta, of obtaining, in this manner, a declaration of opinions, which, by their intrinsic inconsistency and absurdity, would tend to elicit the truth, and to refute themselves. The following queries were accordingly drawn up with this view; and the (Proto-Medico) or President of the College of Physicians was directed by the Government, to furnish me with answers¹.

¹ After the abolition of the order of Jesuits, who had the direction of public instruction, the Grand Master, Pinto, thinking it necessary that their functions, in that capacity, should be supplied, established, in 1771, an university, of which a College of Physicians, consisting of eight members, formed a part. This college at present consists of ten members, of whom the President is entitled Proto-Medico. The appointment to this situation used to belong to the Grand Master. Since the destruction of the order of Knights of Malta, one Proto-Medico has been removed, and another appointed, in his place, by Sir Alexander Ball. The name of the actual President is Caruana. The members of the University have lecturing rooms in the upper part of the building, which belonged to the Jesuits, in the Strada St. Paulo; whilst the lower part has been converted into an Exchange. Some of the funds belonging to that order have been reserved for the University.

CHAP. XXI.

Queries proposed to the College of Physicians of Malta, respecting the Pestilences that have occurred at that Island—and Answers of the Proto-Medico, or President of the College.

QUERIES PROPOSED TO THE COLLEGE OF PHYSICIANS OF MALTA.

QUERY 1. At what periods, and to what degree, has the disease, called plague, been known to occur at Malta?

2. By what symptoms has it, at each of those periods, been distinguished?

3. What organs were primarily, and most severely, affected?

4. To what circumstances has it been attributed as its cause?

5. Do you consider its propagation, by means of a contagious virus, as having been, on any of those occasions, distinctly ascertained?

6. What were the states of the atmosphere, winds, and weather, previous to the commencement, during the continuance, and after the cessation of the malady?

7. At what time of the year did the disease, at each period of its occurrence, commence and terminate?

8. Were any, and what, means employed for its cure?

9. Were these means, and to what degree, effectual?

10. What proportion did the number of persons affected, upon any occasion, bear to the whole community?

11. What proportion did the number of deaths bear to that of the sick?

12. Were any, and what proportion, of the recoveries which took place, attributed to the efficacy of the medical treatment pursued?

13. Have you known medical men, or others, to expose themselves, with or without precautions, to contact with the sick?

14. What means have you known to be employed by attendants upon the sick, with a view to their personal protection?

15. Has any, and what degree of, efficacy, been attributed to these precautions?

16. Have you known persons, without taking such precautions, to expose themselves with impunity, to contact with the sick?

17. Is it your opinion, that the infection is communicated by contact with the body, or with

the clothes, or that it is carried in the atmosphere? And, if the latter, at what distance from its source do you consider it capable of acting?

18. Have you known an instance, when a husband and wife have been living together, of the one having died of the plague, while the other has remained in health?

19. Have you known an instance, in which a child, at the breast, has remained in health, while the mother, who suckled it, during the whole period of her illness, has died of the plague?

20. Do you consider the body of a person, dead of the plague, capable of communicating infection?

21. Have you seen the bodies of persons, dead of the plague, dissected, and what appearances have they exhibited?

22. Have you remarked whether, in individual cases, the phænomena of the disease appear at uniform periods from the application of the cause, and whether there be any uniformity, in its duration and course?

23. Is it your opinion, that diseases, which are known to arise from specific contagion, in some instances, may, in others, arise from other causes?

24. What are the usually prevailing states of the winds and weather in this island, during the different seasons of the year; and what influence

have you, in ordinary seasons, observed them to exercise over the living body ?

25. What classes of the community, in pestilential seasons, have you observed to be most exempt from, or most obnoxious to, the prevailing malady ?

26. Will you be so obliging as to state any facts relating to this subject, which may have come within your personal knowledge, but are not involved in the preceding questions ?

27. What are your opinions, or knowledge, respecting the susceptibility of various articles to imbibe, their capability to retain, and their power to communicate infection ?

28. What proportion of those persons employed in the expurgation of goods, in the Lazarettos, have you known to be seized with plague ?

29. Have you known instances, and how many, of the same person having the plague more than once, and how often ?

30. What do you consider the distinguishing and characteristic symptoms of that malady ?

31. How many persons have you known to have been actually in contact with persons ill of the plague ; and how many of these have been seized with the malady, and how many have remained in health ?

32. Have the British or Native inhabitants

been proportionably most exempt from the malady?

33. How do you account for this difference?

Valletta, 28th June, 1815.

ANSWERS TO THE PRECEDING QUERIES, BY LOUIGI CARUANHA, PROTO-MEDICO, OR PRESIDENT OF THE COLLEGE OF PHYSICIANS*.

Answer 1. The plague in Malta was observed to vary as to time and degree without the least regularity.

2. The incipient symptoms were chiefly inflammatory, with atra bilious vomiting, acute head-ach, and delirium; after the third day, or fifth, or seventh, or even after the eleventh, some had inflammatory swellings of the parotid; others

* Not choosing to be considered responsible for the accuracy of this translation, I annex the answers in the original:—

Responsa 1. La peste in Malta s'osservi di periodo, e grado variabile senza la minima regolarita.

2. I sintomi, che s'osservarono nel principio della manifestazione della malattia erano per lo piu infiammatori, accompagnati da vomito d'atrabile, da dolor intenso di capo, e da delirio; dopo il terzo giorno dello sviluppo della malattia, o dopo il 5to, o dopo il 7mo, o anche dopo 11mo,

of the subcellular ; and others of the inguinal glands. Some had spots, of greater or less size red at the beginning, and afterwards livid ; others had carbuncles, of a bad appearance ; and some had all these symptoms. When the plague was at its height, it was observed to be of shorter duration ; the first inflammatory symptoms were succeeded by great prostration of strength, and the delirium became excessive. Some died suddenly, without any previous symptoms ; but, after death, were covered with spots, swellings, and livid marks, in different places ; but more especially towards the groin ; and their bodies became putrid in the course of a short time. Sometimes,

si vedevano in alcuni individui tumefattè, ed infiammate le glandule parotidi, in altri le sotto ascellari, in altri le sotto inguinali, in altri la comparsa delle petecchie più, o meno larghe, al principio rossegianti, ed indi lividanere ; in altri i carbunchi, e questi quasi sempre d'indole maligna, ed in altri finalmente si osservavano uniti quasi tutti i predetti segni. Nel tempo della maggior accensione della malattia i periodi della medesima s'osservarono più brevi, ed i primi sintomi infiammatori subito si cambiavano in una grande, e somma prostrazione della natura, ed il de arrio diveniva totalmente furioso ; alcuni poi morivano subitaneamente senza aver dato precedentemente alcun segno della malattia ; ma dopo morte osservati i loro cadaveri si trovavano pieni di petecchie, vibici, e striscie lividonere in varie parti del corpo, ma più verso gl'inguini ; ed i loro cadaveri presto s'incancreuivano intieramente. Qualche volta poi s'osser-

sudden death was not followed by any marks, and more especially in aged people. But after some days, when the rest of the family had been put in quarantine, the plague would break out on various individuals, with evident symptoms; and some of these died. Towards the decrease of the disease, it became of longer duration, the symptoms milder, the cure easier; and it sooner appeared after infection.

3. The organs chiefly affected were—the head, the parotid, subcellular, and inguinal glands, and the skin; but more frequently, and severely, the inguinal and subcellular glands, and the skin.

4. The origin of the plague, in Malta, was not known; but it may have originated from the

varono succedere morti subitanee, senza nemmeno osservare alcuna marca nel cadavere, e per lo più nelle persone attempate, passati poi pochi giorni, non ostante che il resto della famiglia si teneva sotto la più stretta quarantena, si manifestava la malattia sugli altri individui della famiglia colli sintomi più chiari della peste, ed alcune di tali famiglie si sono estinte intieramente. Nella declinazione della malattia i periodi della medesima s'osservavano più lunghi, e più miti i sintomi, e più facile la guarigione, ma più presto si manifestava la malattia dopo il contatto avuto.

3. Gli organi erano principalmente il capo, le glandule parotidi, le sotto ascellari, le sotto inguinali e la cute, ma i più frequentemente, e severamente le inguinali, sotto ascellari, e la cute.

4. Non ho alcuna positiva conoscenza dell' origin della malattia nell' isola; ma sono d'opinione essere uscita dal

Lazaretto, where persons coming from Alexandria had it.

5. The spreading of the plague was by communication with infected persons, or goods.

6. The wind being usually variable at Malta, it was so during the plague. The most frequent winds were from the north.

7. The plague appeared in April 1813, and ended in March 1814. July, August, and September, were the months in which the disease was most fatal.

8. Various remedies were applied, by the different professors, who selected those most adapted

Lazzaretto, poichè precedentemente ivi avea osservato la malattia in persone, che venivano da Alessandria, dove dominava la peste fieramente, ed il commercio di quella città era molto esteso con quest' isola.

5. La propagazione della malattia, secondo la mia opinione appoggiata, e sostenuta da costante osservazione, e stata sempre il prodosso della comunicazione trá gl' sudividi sani, e gl' individue o robe infette.

6. Essendo Malta un isola piana, il di lei atmosfera, ed i venti sono molto variabili; in tempo della peste proporzionalmente, ed altre stagioni spiravano piuttosto quelli del nord.

7. La peste comparve in Malta nel mese d'Aprile 1813, e cessi in Marzo 1814, nelli mesi pero di Luglio, Agosto, e Settembre 1813, era nella maggior accensione.

8. Vari erano i mezzi applicati agli ammalati dalli differenti professori, ma i più accorti tra questi scielivano, e con

to the constitution of the different patients, according to the violence with which the plague might affect them.

9. Where the plague was so violent as to produce death in a few hours, no remedy was found available ; but when its period was longer, vomits appeared to have good effects, at the beginning, blood-letting, and sudorifics, followed by bark, in large doses, camphor, mineral and vegetable acids, and the like.

10. The plague being propagated by communication, no proportion was observed in respect to the population.

vantaggio sempre quelle mezzi, che più vedevano, e congetturavano essere indicati dalla particolare individuale costituzione del paziente, e dalla maggiore, o minore violenza, con cui il veleno pestifero agiva sulla costituzione degli stessi pazienti.

9. Quando i periodi della malattia erano così brevi, che dopo poche ore succedeva la morte, non s'osservò alcun mezzo proficuo ; quando però i periodi erano più durevoli, e distinti si ricavo gran vantaggio dal vomitivo dato al primo apparire della malattia; da qualche emissione di sangue, e dalli sudoriferi; nel secondo periodo di poi dalla china in larga dose, dalla canfora, dagli acidi minerali, e vegetetabili, ed altri simili mezzi.

10. Essendo stata propagata la peste per ragione della comunicazione avuta non s'osservo alcuna proporzione tra la popolazione.

11. There died ninety in the hundred of the sick, during the first period of the disease, and sixty in the hundred, during the second period.

12. The greater part of the cures was by the resolving method, a few by suppuration, and fewer by caustic.

13. I have known divers persons expose themselves to contact with the sick, some with and some without precaution.

14. The methods of precaution were to cover the body with waxed cloth, to rub it with oil, wash it with vinegar, to observe cleanliness, to breathe fresh air, and to fumigate with sulphur.

15. The best method was to cover the persons employed with waxed cloth, and let them breathe fresh air.

11. La proporzione osservata tra i morti, egli ammalati nel primo periodo era di 90 a 100, nel secondo di 60 a cento.

12. Proporzionatamente più guarivano col metodo risolvante, meno col suppurante, ed assai meno col caustico.

13. Ho conosciuto varie persone esporsi al contatto degli ammalati con precauzione, e senza.

14. I mezzi impiegati per cautela degli impiegati cogli ammalati di peste furono il coprere il corpo con tela incerata, l'ungersi d'olio, il lavarsi coll'aceto, il tener la pulizia, il respirare l'aria libera, ed i profumi d'acido solforico.

15. Il più efficace mezzo era il coprire d'incerata il corpo degli impiegati, ed il respirare l'aria libera.

16. Many persons were exposed to contact with the sick, without taking the infection.

17. There is no proof that the plague was communicated by the air.

18. It is known that several women, in the first days of their husbands' sickness, lived, eat, and slept with them, without suffering in their health.

19. Several children drew milk from their infected mothers, up to the period of death, without taking the plague.

20. According to my opinion, a dead body can communicate the disease.

21. No dissection took place in my presence.

16. Moltissime persone conosco essersi esposte al contatto degli ammalati, senza aver sofferto il minimo danno.

17. Nessuna prova v'è che la comunicazione della malattia, istata per mezzo dell'atmosfera.

18. So dalla relazione di alcune mogli degne di fede, che le medesimo nei primi giorni della malattia dei loro mariti, vivevano, mangiavano, dormivano colli medesimi, senza d'essere stato mai soppraggiunto alle medesime alcun incomodo di salute.

19. Ho conosciuto varj ragazzi succhiare il latte dalle loro madri in tempo, che queste erano ammalate di peste, e così continuare sino la morte, senza che alli primi sia succeduto alcuno male.

20. Secondo la mia opinione il cadavere può dare l'infezione.

21. Nessuna sezione anatomica fu fatta in mia presenza.

22. The periods, at which the disease made its appearance, in various individuals, after communication, were various. It was generally from the third to the sixth day, sometimes longer, even to the fourteenth day, but not later.

23. My opinion is in the affirmative.

24. The winds are very variable at all times; but, generally, in the autumn, we have south winds; and, when these continue for several days together, the inhabitants suffer great heat, with continual, or intermitting, or sometimes contagious fevers.

25. The lower orders of the people were most liable to infection.

26. It was observed that divers individuals did not take the disease at one moment of contact,

22. Nessuna uniformità di periodo s'è asservata nella comparsa della malattia sugli individui dopo la comunicazione avuta, si può però assicurare, che per lo più appariva tra il 3^{zo} e 6^{zo} giorno, sebbene alcune volte tardava di più, e qualche volta tardò fino al 14^{zo} giorno ma non più.

23. Sono d'opinione affermativa.

24. I venti sono variabilissimi nell'isola in ogni stagione, ma per ordinario nell'autunno spirano quelli del sud, e spirando questi per non interratti giorni, si soffre dagli abitanti un eccessivo calore, essi sviluppano delle febbri continue, o intermittenti, e qualche volta contagiose.

25. Meno esente d'acquistare la malattia fu la bassa classe del popolo.

26. S'è asservato, che varj individui non ebbero la malat-

but took it at another, at a subsequent period, and died from the contagion. Some had the disease so light, that they continued to act as if they were in health. All women with child, excepting two, miscarried: after delivery, they appeared relieved, but a few hours afterwards all died. Some persons were observed, after the plague, to remain chronically affected, without communicating the contagion, during their chronic state. It was observed, during the plague, that all other sickness ceased, and that chronic valetudinarians got better; and even some are to this day free from the diseases they had before. Since the plague, it should be observed, that the

tia en una data occasione di Contatto, ma l'ebbero in un'altra dopo qualche tempo, erestarono vittima del contagio. Ho conosciuto alcuni i quali ebbero la malattia così leggera, che permetteva loro di continuare ad agire come sani. S'è osservato che tutte le gravide ammalate di peste, all'eccezione di sole due, abortirono, dopo l'aborto si mostravano sollevate, ma poche ore dopo morivano tutte. Si sono osservate varie persone, dopo la peste avuta, restare cronicamente affette, seryperó dare ad altri il contagio in tempo del loro cronicismo. S'è osservato che nel tempo della peste cessarono tutte le altre malattie, e gli Valitudinari cronici starsene sempre bene, ed alcuné di questé continuano fin oggi essere liberi dalli loro premieri incomodi. Vado osservando finalmente che dalla peste in qua sono più frequenté le malattie di consunzione, e di delirio malinconico.

more frequent diseases are consumption, and melancholy delirium.

27. According to my opinion, and knowledge, any thing in contact with a plague patient is infected. Some, however, may be soon and easily purified, while others cannot. It appears, therefore, that some are susceptible in the first degree, others in the second, and others in the third; and their degree of susceptibility, and their power of retaining or communicating infection, is in proportion to their porosity.

28. During the period of fifteen years, in which I frequented the Lazaretto, no cargo arrived, the depuration of which infected the people. The general purification of the island was effected by severe and decisive measures, and generally by

27. Secondo la mia opinione, e conoscenza qualunque cosa in contatto d'un appestato i infetta: molte però di queste si possono presso, e con facilità purificare, ed altre no; quindi e che alcune sono suscettibili di primo grado, altre di secondo, altre di terzo, &c. e tale loro grado di suscettibilità, e loro forza di trattenere il contagio, e di comunicarlo va in proporzione alla loro maggior, e minore porosità.

28. Da quindici anni, che pratico il Lazzaretto non i giunto mai un carico, dal quale restarono infetti i depuratori in Lazzaretto. In tempo della depurazione dell' isola in generale si sono prese misure dele più secure e decisive, e queste vennero esercitate da persone per lo più spestate, e perciò pochissime erano le infezioni avute in tale operazione.

persons who had had the plague. The accidents therefore of this operation were few.

29. No person had the plague more than once. I am, however, assured by a medical man, that he knew one person, who had the plague twice. The persons who had had the plague, and were employed, sometimes had carbuncles; but they were slight, with mild symptoms.

30. I am not acquainted with the pathognomonic symptoms of the plague; but I had never any doubt of the disease, in cases where opinions differed, guiding myself by what I have observed in No. 2, with other circumstances resulting from practice.

29. A mia cognizione nessuna persona ebbe la vera peste più d'una volta, sono però assicurato da un medico d'aver osservato egli la malattia per due volte sul medesimo individuo in uno degli spedali di peste di questa città Valletta. Posso poi assicurare, che nella superficie esterna del corpo degli spestati addetti al servizio infetto di tanto in tanto osservava delli furunculi portanti i caratteri di quelli detti pestilenziali, ma questi erano sempre benigni, e leggermente accompagnati dalli sintomi generali.

30. In quanto a me non saprei determinare quale sia il sintoma patognomiconico della peste, ma non mi sono trovato mai ingannato nel caratterizzare la malattia di qualcheduno, sì di cui nascerano delle ambigue opinioni, quando mi regolai dalla comparsa di quelli sintomi da me già narrati nel No. 2, dalle concomitanti circostanze, e dall'occhio che mi trovo aver acquistato dalla prattica.

31. During the plague, many people conceal their communication with the infected; and, as the number of these persons was not known, it is impossible to calculate the proportion required. In respect to persons employed in the service, in actual contact with the sick, they all got the plague, and only a few of them were cured; while those who had it before continued well.

32. The plague does not, in my opinion, pay any respect to nations. Those persons keep well, who know what precautions to use, and are in a situation in life to adopt them.

Malta, 3d July, 1815.

31. In tempo, che corre la peste in un paese, varie persone per non soggiacere al rigore delle leggi di quarantena, non palesano la comunicazione avuta coll' infetto, il chè é una delle principali sorgenti della propagazione della malattia; quindi in Malta essendo succuduto lo stesso per qualche tempo, non sapendo nelle case, quali erano le persone in attuale contatto col infetto, non si poteva calcolare la ricercata proporzione. Di quelle persone poi addette al servizio degli spedali, in attuale contatto cogli ammalati tutti hanno avuto la malattia, e pochissimi di questi si sono guariti, all eccezzione degli spestati, quali comparivano stare quasi sempre bene.

32. Secondo la mia opinione la peste non porta riguardo ad alcuna delle nazioni, ed in generale in Malta restarono sani tutti quelli, i quali hanno saputo, e potuto usare, e praticare le necessarie precauzioni.

Malta, li 3 Luglio, 1815.

CHAP. XXII.

REMARKS ON THE PRECEDING ANSWERS.

IT may be useful that I should offer some comments upon the nature and tendency of the answers, which have been returned to my queries, as they afford such striking examples of the manner, in which the believers in contagion first deceive themselves, and afterwards endeavour to mislead others. I would be understood to impute no improper intention to the President of the College of Physicians of Malta, who has done no more than others, in allowing himself to be misled by the unexamined delusion of 270 years. But it is not the less necessary on that account, that they should be rendered palpable to the world.

1st. The first query has been entirely misunderstood, and supposed to refer to the duration and malignity of the disease in individuals, instead of the periods and severity of its occurrence in the island. By reference to history, I find that the plague committed terrible ravages at Malta, in 1592, and 1676; at which latter period,

one village only remained free from the distemper, and is therefore called Sasi, which signifies pure *. I do not learn, that it has occurred with severity since 1676, until its recent attack in 1813. If this be a fact, the inferences, which it supplies, are of very great importance. In the first place, it shews, that, in former times, Malta has been more subject to the plague, than in modern days. 2dly. It shews, that a country remaining even for a century and a half exempt from epidemic diseases, affords no guarantee of its continuing always to enjoy a similar exemption; and 3dly, the circumstance of Malta having been afflicted with the plague but three times in the space of two centuries; whilst Gibraltar, and many parts of Spain, being in similar intercourse with Turkey, and at a greater distance from it, as well as under a similar system of precaution, have been afflicted with the malady four times, in the course of fourteen years, (from 1800 to 1814) would of itself constitute a sufficient ground, if there were not so many others, for entirely rejecting the doctrine of contagion.

2. and 3. The second and third answers require no comment.

4. and 5. The fourth and fifth are in the true spirit of a believer in contagion, who has taken

* Boisselin's History of the Knights of Malta, p. 56.

the doctrine so implicitly for granted, that he cannot regard it as a fit matter, either for doubt, or for enquiry.

7. In the seventh, there are two errors in point of fact. The disease is stated to have appeared in April, 1813, whereas it began, according to Mr. Corner's diary, on the 6th of May; and to have ended in March, 1814, whereas it terminated in November, 1813, there being in December, according to General Maitland's proclamation, nothing more than a case of slight suspicion, but which turned out not to be the plague. It is confessed, that July, August, and September, were the most fatal months.

8. In answer eighth, we are informed, that "various remedies were *applied* by the different professors," &c. This is certainly inaccurate. If even fit remedies were *prescribed* by the physicians, what probability is there, that, under the belief in contagion, their directions would be properly enforced?

9. The real meaning of the ninth, is, that, where the disease was severe, the medicines employed were of no avail; but where it was milder, and recovery ensued, credit was falsely given to the treatment.

10. The answer to the tenth query is altogether an evasion. As in epidemics, the number of

the affected generally bears but a small a proportion to the whole community, the proper solution of this question would have borne too hard upon the doctrine of contagion. It is seldom, that, even in the most destructive pestilence, one-fourth of the people are seized with the prevailing malady.

11. The recoveries were very few. It appears, that, during the violence of the disorder, there recovered only at the rate of one in ten, or ten per cent. of the whole sick; and, at its mildest period, only at the rate of forty per cent. Respecting the intrinsic severity of the disease, it is impossible for those, who were not present, to form an accurate judgment. But, from the description of the practice, it is easy to perceive, that, had it been possible, that, in the existing state of opinions, the prescriptions should have been duly enforced, their application would not have been followed by any material benefits. Of between eighty and ninety sick, who were sent to the Lazaretto hospital, toward the termination of the malady, *two* only survived. These are not presumed to have had any medical treatment.

12. This answer is entirely fallacious. To expect to cure a general disease, by removing a particular symptom, is about as rational, as to

expect to route the main body of an army, by killing some of its videttes.

13. In visiting the sick, the medical faculty were expected, and enjoined to use precautions. And, notwithstanding this answer, it would seem that these precautions were deemed indispensable, all who might venture to touch, or to approach the sick, within a certain distance, being considered as having thereby rendered themselves liable to a rigid quarantine.

14. The precautions used, as stated in the 14th answer, would, even if contagion existed, be better calculated to add to the terror of the sick, than to the security of those by whom they are employed.

15. This question has been entirely misunderstood, or evaded.

16. This fact, would alone be sufficient to shew the impossibility of the disease depending upon contagion.

17. "There is no proof that the plague was communicated by the air." Unless proof and assertion be synonymous, there is certainly no proof that it was communicated by any thing else.

18. and 19. Each of these answers contains in itself, as far as it goes, confirmation of the doctrine of non-contagion.

20. This is indeed hazarding a great deal in support of an opinion.

21. Dissection of the bodies of those that died of the plague, was prohibited by the Council of Health, probably under the apprehension, that, by such means, the disease might be propagated. This conjecture appears to be farther confirmed by the preceding answer.

22. This is a point respecting which no two of the advocates of contagion have ever been able to coincide. The periods have been variously represented, from the moment of touch, to sixty, seventy, or eighty days.

23. The opinion that diseases, which arise, in some cases, from contagion, may, in others, depend upon other causes, is an absurdity necessarily emanating from the hypothesis of contagion, in epidemic diseases: for, without this supposition, the inconsistency of the doctrine would, if possible, be still more glaring.

24. During my stay at Malta, from the 24th of June to the 9th of July, 1815, Fahrenheit's thermometer generally ranged from 73° to 76° in the shade, about eight o'clock in the morning; and on the 5th, 6th, and 7th of July, from 78° to 80° , only a few degrees lower than at the height of the pestilence in 1813. From this, it would appear, that the noxious influence of the air is not determined by degree of temperature. It seems principally, although not solely, to depend upon its vicissitudes.

Abstract of the daily Vicissitudes of Temperature, according to Mr. Corner's Diary, in the Year of the last Plague at Malta, (1813.)

On the days not specified, the vicissitudes are at, or under 6° , but by far the greatest proportion of the latter.

In January, the vicissitudes were four days 7° , three days 8° , one day 11° , and two days 13° ; but the 11° was calculated to midnight, and one of the 13° from five o'clock in the morning.

In February, there were two days 7° , one day 8° , and one day 11° , which last was also calculated to midnight.

In March, the vicissitudes were nearly the same as in February.

In April, they were somewhat increased: but the number of days that they exceeded 6° , were, in those three months, similar, viz. four.

In May, the vicissitudes were as follow: one day 7° , one 9° , two 10° , two 11° , two 12° , one 13° , and one 14° . The plague appeared on the 6th of this month.

In June, the vicissitudes were, one day 7° , three days 8° , five days 10° , two days 11° , and one day 13° , exhibiting the greatest sum of vicissitude.

On the first four days of July, the vicissitudes were at their extreme point. On the first, the

thermometer ranged from 70° at six o'clock *a. m.* to 85° at three *p. m.* being a difference of 15° . On the second, if it had been noted at the same hour in the morning, and supposing it to have been at the same temperature as the day before, the variation, as the highest degree was 87° at five o'clock, would have been 17° . On the third, the extreme height of the thermometer being 88° , upon the same principle, taking its height at six *a. m.* at 70° , the variation would have been 18° ; and on the fourth, the thermometer having been at one o'clock at 85° , we may conclude, that the variation, had the thermometer been observed at six o'clock, would have been 15° , as upon the first day. But it was not noted, upon the second, until eight o'clock, nor, upon the third and fourth, until nine. Thus we may consider the vicissitudes on two of these days to have been 15° , on one 17° , and on one 18° . At this period the disease was the most diffused and fatal; but, as the month advanced, the temperature again became more equable: and, after the sixth, the variation does not appear on any day to have exceeded 8° .

In August and September, the variations exceeding six degrees were still frequent and considerable, although not so numerous as in June, nor so excessive as in the beginning of July. During these two months, the disease had de-

clined considerably ; it was but little heard of in October, and wholly ceased in November. In this latter month, the vicissitudes of temperature did not, excepting for two days, exceed 10° . In December, they rose above 6° , only three times. This month, only one case occurred, which was considered suspicious ; but the suspicion proved unfounded.

25. The following circumstances are in confirmation of my theories respecting the different degrees of liability of different portions of the community. Dr. Francis Leon Gravagna died of the plague at Malta, and Dr. Mac Adam at Gozo. Mr. Burdon, a practitioner of Valletta, was affected with the disease ; but recovered. Mr. Saunders, an apothecary of Valletta, attended the sick, under *surveillance* ; and was not affected with the malady. Dr. Gravagna was attended during his illness, with precautions, by Mr. Green, Deputy Inspector of Hospitals. Mr. Green had not the disease. Many other medical men escaped. They were exempt, I believe, in a somewhat greater proportion, than other descriptions of persons of the same rank in society ; certainly in a much greater proportion than the clergy ; for, whilst only two physicians perished, the clergy lost twenty-six of their number. These facts are totally inexplicable, according to the doctrine of contagion ; since, if that doctrine were correct,

the medical faculty being first and principally exposed, would be the greatest sufferers; but very easily accounted for, if we presume the air to be the cause of the disease, since no other circumstance, of common occurrence, is so well calculated to propagate the maladies arising from that source, as exposure to the exhalations from churches and burying-grounds, in which the clergy are so frequently called upon to officiate. To the honour of the Maltese clergy, let it be recorded, that they performed Church Service, attended burials, and discharged punctually all the other duties of their calling, during this momentous period. The consequence was, as I have stated, that they lost twenty-six of their number; probably a much greater proportion than perished of any other description of persons, even the poorest and most wretched part of the population. Padre Evangelissa, a capuchin of the convent of Floriana, was mentioned as a rare, or singular instance among them, of a person who had had the plague, and recovered.

Dr. Mac Adam's illness was, of course, generally attributed to his having been in contact with the sick, without precautions. But his servant stated, that he had not visited the sick for twenty-eight days previous to his being attacked with the malady; and imputed his illness to his having rubbed himself, whilst warm, with a jacket,

which he used to wear in visiting the sick, but which had been, for some time, hanging up, unused. These allegations, if it were not impossible that either of them should be true, would mutually destroy each other. If the supposed matter of contagion, transmitted in the jacket, could, after a lapse of twenty-eight days, have been capable of propagating disease, it must *à fortiori*, have been still more capable of propagating infection by direct contact, whilst that matter was yet fresh, with the patients, who had communicated the matter to the jacket ; or rather it could not fail to have produced it. And if infection had taken place directly from the patients, I know of no ground, upon which it can be presumed, that it could have remained latent, or inactive, for twenty-eight days. Besides, if the disease were actually contagious, in whichever way it might have been communicated to the master, it could not fail to have reached the servant, which did not happen.

26. The observations contained in this answer are worthy of attention. If the phraseology of contagion had been omitted, they would have been the most genuine, and the least biassed, of the whole.

27. Here we have the doctrine of the susceptibility of goods for infection, in all its absurdity.

28. It is a remarkable fact, that, a smaller pro-

portion of those, who have been employed to *expurgate* goods, in the Lazarettos, have been known to be affected with plague, than of other inhabitants. Deputy Inspector Grieves, who had the very best opportunity of knowing, informed me that, during the plague of 1813, none of the persons so employed were affected. And the Proto-Medico, who is a decided contagionist, confesses, in his answer to my 28th query, that, during fifteen years that he has frequented the Lazarettos, “no cargo arrived, the depuration of which *infected* the people.”

The inference from this acknowledgment is of a nature too important to be slightly passed over. Here we have a man, educated in the belief of contagion, accustomed to the routine of Lazarettos for fifteen years, and confirmed by habit in all the errors and delusions of the prevailing system, avowing reluctantly, that not one of all the persons employed in depurating the numerous cargoes, which, during the whole of that period, had been brought to the Lazaretto, from Turkey, or the other countries suspected of being liable to contagious diseases, had been seized with the plague; the malady notoriously existing, in some of the countries in commercial intercourse with the island, almost every year. Thus, even if plague were admitted to be capable of being propagated from person to person, we should have

here strong ground for disbelieving its capability of being propagated by goods ! But the impossibility that epidemic diseases can depend upon contagion, being demonstrated, it would be supererogatory to enter into any argument, to shew, that this imaginary contagion cannot be propagated by goods.

29. No fact can be better established than the capability of the same person being affected repeatedly by the plague. It has obtained universal belief, both from the palpable nature of the examples, which are daily to be met with, and from the known laws of life.

30. There can be no distinguishing, or characteristic symptoms of a disease, which attacks so many different organs, and in such an infinite variety of degrees.

31. This is a question, which ought not to have been asked, because its solution is impossible. The manner, in which it is attempted to be answered, shews the decided bias of the mind.

32. The answer to this question might be construed into an attempt at repartee. But, taking it literally, it is so far from being correct, that the plague pays a great deal of respect, not only to nations, but to occupations, and individuals.

In the reports of the Council of Health, I observed frequent allusion made to "a foreign physician." Upon enquiry, I found that this

“foreign physician,” was a Jew practitioner of Smyrna, called Lyons, who had been sent for to that place, by the government of Malta, at a considerable expence. It did not appear that this person’s prescriptions were of any efficacy ; but, as he ventured into more free communication with the sick, than the members of the faculty, his intrepidity might have been of some small service, by affording to the afflicted faint glimmerings of hope.

The positive decay of the commerce of Malta, has been the immediate consequence of the dread, inspired, more by the measures necessarily resulting from the erroneous opinions entertained upon the subject, than by the actual calamity ; and the continuance of these restrictions, even in the absence of pestilence, if they have not the effect of destroying that commerce entirely, will, at least, prevent the island from becoming that grand depôt of British trade, in the Mediterranean, for which it is so peculiarly fitted by its position. Of the truth of these observations, no farther evidence need be adduced, than the representations recently made to the Secretary of State for Colonies, by the British merchants of that island.

I shall conclude this article, with the regulations, issued at Malta, upon this occasion ; which, if I be not greatly mistaken, would be deemed,

by the public in general, even supposing the doctrine of contagion were correct, much more mischievous in their effects, than any ordinary pestilence.

REGULATIONS (*referred to in the Proclamation under date of the 24th of December,*) for the Organization of the City of Valletta and Floriana for the ensuing Year, with a view to prevent a return of the late unhappy Malady; and in the event of its breaking out, in any solitary instance, to arrest its progress the moment it appears, which Regulations are also to be carried into effect in the strictest manner by all the other Towns, Casals, and Places in these Islands, as far circumstances may admit.

Division of the Cities.

1. The city of Valletta shall be divided into twenty-four districts, leaving the streets Reale, St. Christoforo, Levante, and the whole of the Line Wall, free for the communication, and passage of carts for the purpose of provisioning the town, moving the sick into the Lazaretto, removing infected goods, &c.

2. The Marina shall be divided equally into two districts, and the town of Florian into seven.

Organization of the Districts.

1. Each district in Valletta shall have one deputy, one clerk, one sick searcher and one serjeant of police, all of whom are to be exclusively attached to such district, and to reside therein.

In Florian, there shall be one deputy and one clerk in each district, and generally such a number of sick searchers, and serjeants of police, as may be required.

2. Independently of these officers appointed generally to the superintendence of the district, there shall be in each street an officer, styled Capo di Strada, under the deputy superintending the district.

3. A due proportion of shops from the public market are already selected, and will be moved into any district the moment it is shut up, for the supply of the necessary articles.

4. The Most Rev. the Archbishop and Bishop has already arranged, that on any district being shut up, a chaplain shall be appointed to administer the sacraments within such district.

5. The Proto-Medico will order into any district, shut up, the necessary medical attendance, to remain therein, with such medicines as may be requisite.

Duty of the General and District Officers.

1. The Inspector General of Police, who will receive his orders direct from His Excellency the Governor, shall be head of the whole police in the island, and shall give such orders and instructions upon all points connected with the police, as the nature of the case may require.

2. Under the Inspector General there shall be two deputies, whose orders, as coming from him, are strictly to be attended to; and in Valletta there shall be, in addition, three adjutants, the one to be considered as adjutant for the Marina, and that part of the town to the southward of Strada Reale, the second of Strada Reale, and the districts to the northward, and the third of Florian, whose orders, as coming from the Inspector General, are equally to be attended to.

3. The deputy of each district has, under the Inspector General, the charge of the police in his own district. He shall forthwith, with the aid of his clerk, make out an accurate return of the whole of the population within his district, and he will take care, that, at the door of each house, there be affixed a list of all persons residing therein, which list is to be corrected weekly, and a copy thereof regularly transmitted to the Inspector General.

It shall be the duty of the deputy frequently to call forth the inhabitants of such houses, to see that they are in perfect health, and he will take care that the sick searcher under him be constantly and unremittingly employed in enquiring into the health of the district, and that regular reports thereof be sent to the Inspector General's office, every three days, when no case of sickness occurs, but when such does occur, a report must instantly be made to the Inspector General, who will communicate the same to the Proto-Medico, that the necessary measures may be forthwith adopted to ascertain the nature of the complaint; and the latter, with this view, will appoint regular medical practitioners to superintend the health of the various districts.

The said deputy will equally take care that the Capi delle Strade under him do, in their respective streets, make similar visits, and regularly report to him the occurrences therein.

Forms of the different reports to be made will be transmitted to the deputies by the Inspector General.

4. As nothing is more materially conducive to the preservation of health, and the prevention of Contagious Infection, than the utmost attention to cleanliness, the deputy of the district is particularly charged with this essential object; and the serjeant of police under him will consider

this as a most important part of his duty. No dirt is permitted to be thrown into the streets, but must be carried to such places as shall be pointed out by the superintendant of the streets, and sanctioned by the Inspector General.

The deputy, the sick searcher, and the serjeant of police, will make frequent visits into all lodging houses, magazines, stores, cellars, and all low buildings, and report whenever they find any such buildings not perfectly clean, sweet, and in proper order, to the Inspector General, who is charged with enforcing due attention to this most important object.

*Mode of proceeding in the Event of Suspicion,
or of actual Plague.*

On any case of suspicion occurring the whole population of the town will, on proclamation thereof being made, repair to their respective houses; the gates of the city will be shut; all public business of every kind is to cease, and no person is to move out of his house except specifically employed by government on the occasion, until the necessary measures for general security can be adopted, when notice will be given thereof.

A small establishment of whitewashers, expurgators, and *beccamorti*, will be maintained in the Lazaretto, for the purpose of moving the

sick and expurgating the houses. And it is clearly to be understood, that the moment any case of strong suspicion or plague happens, the parties infected, and all persons suspected, are equally to be sent to the Lazarettos, taking with them such articles of household furniture or valuables as they may wish to save, of which carriage will be provided ; and that the moment they are removed, the said whitewashers and expurgators do proceed to purify, expurgate, and to whitewash the said house or houses.

The district in which the case may have occurred will be forthwith shut up, and regular barriers established. The deputy will put the street in strict quarantine from the rest of the district, and provision will be made for furnishing the necessary supplies.

*Duty of the Medical Department under the
Proto-Medico.*

In every other disease, where it only affects the individual who may labour under it, it is equitable that he should choose his own medical practitioner, who, of course, will follow his judgment, both with regard to the nature of the malady and the remedy to be applied, but as unfortunately in the instance of plague, the effect of the disease attaches to the whole of the community at large, it is impossible to trust so

serious a charge to the knowledge of any single individual, however learned in his profession, either in relation to the character of the disease, or to the measures to be adopted.

Regular consultations must invariably be held, which shall be composed of a smaller or greater number of members, as the nature of the case may require ; but in no instance can the decision of any serious disorder be left to the opinion of one individual. It thence follows, that all medical practitioners within these islands shall, in every case of grave disorder occurring, within the period above alluded to, report the same forthwith to the Proto-Medico, to the end that a consultation be immediately held to fix and ascertain the character of the disorder, and any omission, on the part of any medical practitioner of whatever nation, of this most important part of his duty, will be held as incapacitating him from hereafter performing any of the functions of the medical department within these islands.

On the same principle it becomes indispensably necessary that a medical examination of all dead bodies, previously to interment, shall take place, which will be directed by the Proto-Medico, and no person upon any account is to be interred without such antecedent examination.

The other towns and casals are to be forthwith organized on a similar principle by the dif-

ferent officers under government, and a report of the exact organization to be sent, without delay, to the Inspector General of Police, for approbation.

By Command of His Excellency,

F. LAING,

CHIEF SECRETARY TO GOVERNMENT.

*Valetta, Palace,
3d January, 1814.*

The vanity and selfishness of the precautions, which have generally been employed on such occasions, and which, had the pestilential constitution of the air been a few degrees more intense, would have here produced consequences still more destructive, are thus happily depicted by the poet of the Seasons :

“ But vain their selfish care ; the circling sky,
The wide enlivening air, is full of fate ;
And struck by turns, in solitary pangs
They fall, unblest, untended, and unmourned.
Thus o'er the prostrate city black despair
Extends her raven wing, while, to complete
The scene of desolation, stretched around
The grim guards stand, denying all retreat,
And give the flying wretch a better death.”

BOOK VII.

RESEARCHES AND EXPERIMENTS AT CONSTANTINOPLE.

The baleful charms
Of superstition there infect the skies,
And turn the Sun to horror.

AKENSIDE,

CHAP. XXIII.

Departure from Malta—Satisfactory nature of my Researches there.—Turkish tribute raised in the Morea, at the point of the bayonet—Twenty persons hanged of a morning by the Captain Pacha, of his own simple authority—Mr. Shoolbred's family embark for Smyrna—the plains of Troy—the Dardanelles—Arrival at Constantinople—My reception—Fire at Galata—Josepini's Hotel—Accounts of the Battle of Waterloo—Visit Pest Houses—Memorialise the Porte—Rumours of Plague—Visit Mr. D'Italinsky—My memorial favourably received by the Porte—The Greek Pest Hospital, near the Seven Towers, ordered to be prepared for me—examine the place—its disadvantages—Professions of the Directors—Advice of Sir Robert Liston—Interpreter—Interview with the Reis Effendi—Mrs. Arbuthnot's Tomb.

ON the 9th of July, I quitted Malta, highly gratified by the results of my investigation, as well as by the hospitality and friendship which I experienced, during my residence at that place.

If, in directing my attention to an object, of research, uncultivated, or scarcely deemed accessible, in modern times, I have found myself in the situation of a benighted traveller, in an unknown land, without a ray of indication, to direct his course, my perplexity has been amply compensated, even by the first imperfect glimpses of luxuriant and novel prospects, presented by unexplored regions, to the delighted view. It must have been the case, I presume, with other investigators as it was with me, that, at every step of their progress, they have met with something, which they did not expect, at first setting out, to throw light upon the subject of their enquiries. Not only have my former notions, respecting epidemic diseases, been generally confirmed; but new elucidations have burst upon me, from every quarter; so that the mass of information, which constitutes the results of my researches, in the Levant, exceeds, by far, my first most sanguine expectations. It will not, therefore, be necessary for the purposes of this narrative, and it would be otherwise unfitting, that I should allow myself essentially to digress, from the immediate subject of my investigation. The want of materials will afford me no pretext for encroaching upon the province of the classical, the literary, or the sentimental traveller: and my inclination will not often prompt me to deviate into aberration.

Thus, passing rapidly over that portion of the Mediterranean Sea, which separates Malta from the Dardanelles, without dwelling, even in imagination, upon those scenes, so rich in glorious recollections, which the pen of the poet, and of the historian, the pencil of the artist; or the chissel of the sculptor, have been so often instructively employed to commemorate, or stopping to contemplate the sad reality of the present wretched condition of those once renowned regions, excepting when the prospect of some impending act of flagrant barbarity compels the soul to yield a shuddering attention; as the sight of a Turkish squadron approaching the Morea, for the purpose of enforcing, at the point of the bayonet, the collection of the annual tribute to the Porte¹; or the Captain Pacha, ordering, of his own simple authority, twenty wretches to be hanged, of a morning, whilst he is enjoying his breakfast, I shall proceed to my destination, with the least possible deviation from my course. But the act of barbarity just mentioned, I find it impossible to pass over without an observation. This heart-rending scene was literally passing, whilst we were off the island of Scio, on the

¹ As we were passing between the island of Cerigo, and the Main, we perceived a squadron of Turkish vessels of war, principally of small size, steering for the Morea, on that hateful periodical mission, as I was informed.

20th July, 1815. The persons executed were, to be sure, reputed pirates. But, that a fellow, who has, all his life, pirated *en grande*, should, and perhaps for that very reason, have the power of hanging, without judge or jury, so many miserable devils, who have only done the same thing, in detail, is a most afflicting triumph of iniquity. As he is himself the sole judge, he has only to call any man a pirate, whom he wishes to destroy.

Whilst this tragedy was acting near the shore, we lay to at some distance, in the offing, in order to give Mr. Shoolbred's family an opportunity of embarking on board a polacca, bound to Smyrna.

Upon the return of the boat from the Polacca, the Tiger made sail; and, in two days, although the wind was, for the most part, adverse, we anchored between the island of Tenedos, and the plains of Troy. It would be superfluous, if it were in my inclination, or plan, to repeat for the thousandth time any of the con-

' The Archipelago is much infested by Greek pirates, who, obeying the dictates of the *lex talionis* in its utmost rigor, have often no mercy on the Turks who fall into their hands. Having, a few days before, been driven from our anchors at Tino, we were obliged to bear away for Paros, where, for two days, we were kept on the alert, by rumours of the probability of an attack from a body of these marauders, who had been recently plundering some of the neighbouring islands.

jectures, which have been made by travellers. respecting this celebrated spot. Although we were detained here for two days, I had not an opportunity of visiting the shore. The plain is even and beautiful, from six to eight miles long, and nearly as broad, covered with brush wood. Opposite to the middle, at the distance of upwards of a league, is the island of Tenedos, famed for its wine. Toward the extremity most distant from the mouth of the Dardanelles, stands Alexandrian Troy, in which the ruins of Alexander's hall of audience, resembling those of a modern mansion in England, are still distinctly to be seen. Toward the other extremity, near the hills, is said to be the scite of ancient Troy. And near the shore underneath, some tumuli are pointed out, one of which is said to be the tomb of Patroclus. A muddy rivulet, which falls into the Dardanelles, at some small distance from its mouth, represents the celebrated Scamander. The extremity of this plain, forming the Asiatic side of the mouth of the Dardanelles, is now degraded by the name of Cape Janissary.

On the 24th. we anchored in the Dardanelles, close to the fort on the European side, and not far from Cape Greco, forming the boundary on that side, of the mouth of the canal. We soon raised anchor, and sailed gently through this celebrated passage, with little more than suffi-

cient wind to enable us to stem the current, having leisure to admire the fine and luxuriant scenery of the interesting banks, which inclose the waters of the Hellespont, and form, on either side, the boundary of Asia and of Europe; as well as to observe, and to regret the deplorable brutality of their inhabitants.

After encountering some heavy squalls in the night, we found ourselves, at day light, in the morning of Thursday, the 27th of July, in sight of "the famed city by Propontic sea," and, at eight o'clock A. M. having weathered the Seraglio point, we came to anchor in the hollow of the Golden Horn, so happily described by the eloquent historian of the Decline and Fall of the Roman Empire.

In the course of the forenoon, I paid my respects to his Excellency Mr. (now Sir Robert) Liston, our Ambassador at the Sublime Porte, who had, for some time, expected, and received me with his wonted affability; giving me however to understand, in a manner scarcely equivocal, that he felt some serious apprehensions respecting the state of my intellectual faculty. To such insinuations I had become so well accustomed, in the course of this investigation, that I always received them, as no doubt they were meant, as raillery on the uncommon nature of my pursuit.

I also waited upon Mr. Morier, our consul general, Mr. Pisani, principal dragoman to the British legation, and several British subjects, to whom I had letters of introduction from England. They were all of a similar opinion respecting the hopeless nature of my undertaking; and they were unanimous in considering Constantinople as the worst part of all the Turkish dominions, for attempting such an investigation, with any chance of success. My chief anxiety, on the contrary, was lest an opportunity of trying my experiments should not, that season, occur.

In the afternoon I returned on board the *Tiger*, where I slept, not having got accommodations ashore. This evening we witnessed a fire, which broke out in the suburb of Galata, and consumed, as we were informed, although reckoned a small conflagration, three hundred houses. The Grand Seignior appeared in person, as is customary on such occasions, and issued orders for extinguishing the fire. The scene was uncommonly grand and awful. The boats rowing in every direction, and gliding with extraordinary velocity over the surface of the illuminated canal, the faces of the persons in them being rendered almost distinguishable by the flames issuing from the burning tenements on the banks above, the dismay, and corresponding noise of the people, formed combinations, which, to a stranger, were

truly imposing. But the prospect of a calamity, which would have appalled every other people, were, by the Turks, regarded as matters of little more than common concernment.

On the 28th, by the advice of Mr. Pisani, I took lodgings at the hotel Josepini, in the main street of Pera. Dined at Mr. Morier's, in company with several English gentlemen, and some foreigners.

Saturday, 29th of July, on going to dinner to Sir Robert Liston's, I heard an account of the battle of Waterloo, of which correct rumours had been in circulation at Malta, previous to our leaving that island.

Sunday, the 30th, was favoured by Mr. Frere, the British Chargé d'Affaires, with a copy of the Gazette, containing an account of the battle. Was happy to recognise, in this gentleman, an old Vienna acquaintance, attached to the embassy of Lord Minto.

Monday, the 31st, visited the French and German Pest Hospitals, in the street of Pera. They are small, insignificant, places, under the superintendence of monks. They contained, at this time, no patients. It is respecting one of these that Olivier makes the following observations. " Upon the appearance of symptoms of a heavy illness, the person suspected of having the plague is immediately sent to the hospital,

situated at the further end of the street of Pera, and wholly appropriated to the treatment of that distemper¹. A Maronite monk is charged with receiving the patients, and administering to them such relief as his zeal may suggest. Gratitude is no doubt due to the man who devotes himself to the task of relieving the sick of the plague, who could resolve to live among them, and to pour into their hearts words of comfort. But, with these good intentions, he ought to combine the knowledge requisite for the treatment of this disorder, and to be able, without exposing himself too much, to afford all the succours, which the sick demand, and humanity requires. Unfortunately, however, the cares of this priest have hitherto been confined to giving to the sick some light food, and insignificant potions, and to presenting himself at the door of every sick person, to administer the spiritual comforts prescribed by religion."

The Armenians, and I believe the Jews, have

¹ This was an error into which Olivier, and all travellers who have treated of the subject, must necessarily have fallen, in taking their accounts from the plague dealers of the Levant, since, under the existing belief amongst Christians, they can have no opportunity of judging from personal observation. Pestiferous patients, in these hospitals, or depôts, never receive any treatment, unless to deny them all sustenance beyond rice water, be deemed such.

also pest houses of their own. The Greeks have three.

Dined this day at Mr. Barbaux's, a Member of the English factory, and Treasurer to the Levant Company. The principal topic of conversation, was the recent conduct of Mr. Ruffin, the French Chargé d'Affaires, in lowering the white, and hoisting the three coloured flag, at the palace of the Legation, upon hearing of Buonaparte's progress. The Turkish government, upon being informed of the circumstance, requested him to haul it down; and, upon his refusal, they directed it to be lowered by their own people.

Wednesday, August 2nd, communicated a draught of a memorial to Sir Robert Liston, to be presented to the Porte, it being understood that it was to be fashioned to their taste by Mr. Pisani, first dragoman of the British legation. Dined at Mr. Morier's, in company with Baron Haller, and Mr. Gropius, two German gentlemen, several English, and some natives.

August 3rd. Dined at Sir Robert Liston's, with nearly the same party. Was informed that the Porte seemed to be favourably inclined to the object of my memorial.

August 4th. Dined, for the first time, at my hotel. Passed the evening at Sir Robert Liston's, who handed me a note, which he had just received

from Mr. Pisani, stating, that, on the following day, my Memorial was to be formally presented by the Reis Efendi to the Porte. For several days rumours have existed of some cases of plague in Constantinople; and to-day reports prevail that it has appeared in Dalmatia.

August 5th. Dined at Mr. Black's, a Member of the English factory, with a party.

Monday, August 7th. Went to Buyuckderé by water, to pay my respects to Mr. D'Italinsky, the Russian Ambassador at the Sublime Porte, to whom I had an introduction from his Excellency Count Lieven, the Russian Ambassador in London. He gave me a very cordial reception, and promised that he would do whatever should lay in his power, to forward the object of my voyage.

In returning to Pera, in the evening, I observed, with surprise, the Turkish boatmen pull suddenly towards the land, as if under some agitation, or alarm. One of them, leaping ashore, ran with a vessel to a well at the edge of a burying ground, filled it with water, and returned quickly to his comrades. They all drank with avidity. Upon enquiring into the meaning of this, a servant, who had accompanied me, pointed to the quarter of the horizon where the sun had just set. This, said he, is the season of Ramazan, or Turkish Lent. If the boatmen

had been all dying of thirst, they durst not have swallowed even a drop of water until the sun had set. But, now, they may eat and drink as much as they please, until it again rises.

Tuesday, August 8th. Was informed by Mr. Pisani, that he expected the Porte would decide respecting my Memorial on the following day. Had several invitations to visit families at Buyuk-deré; but, as rumours of the plague continued to prevail, I thought it right to remain on the spot.

Wednesday, August 9th. Dined at Sir Robert Liston's, with a party. In the evening his Excellency received a note from Mr. Pisani, from the Porte, stating, that the Greek Pest Hospital, near the Seven Towers, was ordered to be prepared for me; and that horses would be sent to the water side, on Friday morning, to take me to see the place.

Friday, August 11th. Went, accompanied by Mr. Frederick Pisani, nephew of the principal dragoman, to see the Pest Hospital destined for my residence; but, the day being very hot, we preferred going by water, to riding on horseback. Embarking at Galata, and coasting along the semi-circular part of the town, which is girt by the sea, we passed the Seven Towers, and arrived at a Butchery, which overhangs the banks

of the Propontus, at the place at which we must land. The stench was almost intolerable. Passing through a village, full of carcasses, hides, offals, blood, Turkish butchers, and mangy dogs, we at length reached some fields, and ranging along the walls of Constantinople, within a small distance of the Golden Gate, we arrived, after walking about three quarters of a mile or upwards, at the Pest House.

This building is situated on the borders of a kind of morass, which is used as garden ground, over which, and over a great part of Constantinople, and of Pera, and indeed down the canal of the Bosphorus, from the Euxine or Black Sea, the north-east wind, which so frequently prevails in the autumnal, or pestilential season, charged with the noxious exhalations, by which it is loaded in its passage, blows directly upon the hospital. A few hundred yards farther to the westward, it would have been unexceptionably situated. I felt the full force of the objections. It had been part of my plan, at setting out from England, not to expose myself to a noxious air, such as I found was likely to exist here; but to have the pestiferous patients, if possible, brought to me, in a comparatively pure atmosphere. But in this, as well as in other respects, I found, that, if there was any chance of succeeding in my enterprise, it would not be by

raising obstacles, but by obviating them. On this subject, I merely hinted that the situation of the hospital at Pera seemed to be preferable, both as it was nearer the residence of the English, and the other Frank inhabitants, and in itself a healthier situation : but the hint was not seized, and I did not repeat it¹. Mr. Pisani, the principal dragoman of the British legation, had, on my part, the whole management of the affair. We examined the outside of the building, which, with the exception of the windows, was in good repair, and by no means an awkward structure. We did not enter, because if we had, we could not expect our friends at Pera, to admit us among them, upon our return. We contented ourselves with inspecting the inside through the windows, which were thrown open with some difficulty, and no equivocal symptoms of ill humour, by the Papa, who officiated as master of the ceremonies, upon the occasion. The inside, at least the ground floor, was in a situation that denoted it to be the nightly habitation of sheep and goats. We were attended by one of the directors or governors of the institution. He was overflowing with professions. There was

¹ Mr. Von Rosenfeldt, for the reasons I presume which I have assigned, gave, on a subsequent occasion, a preference to the Pest House at Pera, over that of the Seven Towers.

nothing which I might desire, that should not be complied with. I affected to be satisfied, and told him that I should take possession on the following Monday. In the mean time, he promised to provide a horse and an interpreter ; and, as to cookery, it appeared that I must take my chance of being served by the cooks of the hospital, after they had disposed of the patients.

Dined at Sir Robert Liston's, where we were informed of Bonaparte having given himself up to the British, and of the surrender of Lyons by Suchet to the allied armies, &c. Went to a concert at Mr. Morier's, in the evening.

Saturday, August 12.—Sir Robert Liston advised me to carry no money into the hospital, and to take some pains to shew its inmates that I had none. It was arranged that I should be presented to the Reis Efendi, on Monday, previous to entering upon my investigation. The Tyger sailed for Smyrna. Requested that Sir Robert Liston would be so kind as to admit under his roof such articles of baggage as I did not mean to take to the hospital, which he was good enough to comply with.

Sunday, August 13.—A person called upon me, introducing himself, with great confidence, by means of a few words of bad English, and a few words of worse French, from which I could only collect, that he was destined, by the directors

of the Greek hospital, to be my interpreter. He was a swarthy Greek, a native of Candia, about twenty-eight years of age, and above the middle stature, with much such a countenance as the representative of Rugantino, or the bravo of Venice, upon the stage. He endeavoured to make me comprehend that horses would be ready, at a certain place, on the Constantinople side of the Golden Horn, to convey me to the Pest Hospital, at the time which had been appointed. On understanding that he was the person selected to be my interpreter, I ventured, after asking him a few questions in English and in French, to insinuate some doubts respecting his qualifications; upon which he assured me, with all the vanity which the modern Greeks derive from the reputation of their ancestors, and the ignorance incidental to their actual situation, that he was a philosopher, and a Candiote, and consequently an able interpreter. On representing to Mr. Pisani my surprise that such a barbarian should have been selected for such an office, he observed, that, for a service esteemed so dangerous, no person better qualified could be found. Determined to raise no obstacles, I made no further remonstrance. His christian name is Constantine: his surname I have forgotten. He wore the Frank dress, which he considered a mark of distinction.

Monday, August 14.—Had the appointed interview with the Reis Efendi, or secretary of state for foreign affairs. He extolled my humanity; and I attributed every good, that could arise from my investigation, to the happy star of the Grand Seignior. If the countenance of the Reis Efendi was a true index to his mind, whatever might have been his views as a politician, he must, as a man, have wished well to the enquiry in which I was engaged; for I never beheld more agreeable features, or a more benignant expression. But alas! how often is the man sunk in the politician,

It being the fast of the Ramazan, coffee and pipes were not served as usual at other times. Whilst I was waiting to be admitted to the conference with the Reis Efendi, I observed a person, in the Frank dress, brought in to be examined by the Dragoman Argiropulo, apparently for some act of criminality. He was obliged to take off his shoes, and perform other acts of humiliation.

During this interval, I was engaged in conversation with Mr. Argiropulo, brother of the Dragoman, who speaks very good English, having resided, for several years, in London, as Secretary of Legation from the Porte to the Court of St. James's.

Before I could return from the Porte, it was too late to proceed to the hospital, and I was

obliged to postpone my entry till the next day. Dined at Mr. Barbaux's. In the evening called at Sir Robert Liston's to take leave. My romantic expedition was of course principally the subject of conversation ; and I was a good deal rallied upon it. One allowed me precisely twenty-eight days to live ; and another recommended that I should pitch upon the spot where I should prefer being buried. Choosing to be in good company, I expressed my wish to be buried in the vicinity of the tomb of Mrs. Arbuthnot ¹.

¹ The lady of a former British ambassador to the Porte, who died at Constantinople, much regretted. She was, I believe, removed to England ; but a neat monument is erected to her memory in the burying-ground in the suburb of Pera, where I used afterwards to walk, during my convalescence and quarantine. The tomb-stones in the English burying-ground, I was sorry to observe, were soiled and defaced by the Turkish artillery mules.

CHAP. XXIV.

My servant absconds—enter the Greek Pest Hospital, near the Seven Towers—description of the building—furniture of my apartment—first visit to the plague patients—repetition of visits—combination of functions—innovations found impracticable—opposed by ignorance, obstinacy, and hostility—motives of the Turkish government, of the Directors of the Greek Hospital, and of the executive agents respectively, for desiring the failure of my investigation—ceremony attending the admission of patients—fanaticism of the interpreter—grounds of my expectations of success.

TUESDAY, August 15.—As I was preparing, about nine o'clock in the morning, to set out for the Seven Towers, I found that Nichola Vuchino, the servant, whom I had hired to accompany me to the hospital, had absconded, his heart having failed him. After some enquiry, he made his appearance, and excused himself, by saying, that his wife and father had interfered, and prevailed upon him to abandon his design. It was now too late to procure a substitute; and, as I would not postpone the matter, on any account, it was necessary that I should confide my fate to the servants of the hospital, and the interpreter hired by the directors.

Accompanied by this interpreter, by Mr. Stavrack, one of the directors of the Pest House, and by Piero, Mr. Pisani's servant, I left Josepini's hotel between ten and eleven o'clock ; embarked at Galata, and, proceeding round the Seraglio point, coasted along the south side of Constantinople, to the castle of the Seven Towers, landing at the Butchery as before. We arrived at the hospital about noon.

The persons who had accompanied me from Galata, having retired, after seeing me within the walls, I began to examine the inside of my new dwelling. The building consists of two distinct parts of one story, fronting each other, with a narrow court between them, the whole being surrounded by a high wall. That part which was appropriated to my use faces the north-east ; that containing the patients and the servants the south-west. I found the side of the building which was allotted to me destitute of all furniture, some of the windows being without glass, with frames that either could not be opened, or would not remain shut. Out of the chambers, apartments, or wards, of which that wing consists, I chose one on the upper floor for my residence, opening toward the stair-case, leading to the opposite wing, which contained these hapless patients. The interpreter chose the room adjacent.

For my apartment, a common deal bedstead, and a bed which smelt as if it had been lain on by pestiferous patients, were the sole articles of furniture provided. The interpreter slept on a mattress on the floor. My medicine-chest served me for chair, table, and writing-desk. In the balcony where we were to eat, was an old table, two old chairs, and some benches. Notwithstanding the most urgent representations, it was some days before I could get the bed changed, for one that smelt less offensive. The dinner, with which we were served, was very bad; but my attention was otherwise so much occupied, that I did not think of the necessity of attending to matters of subsistence, as a means of obviating disease.

The necessary arrangements, respecting the apartments, being completed, I next went to visit the plague patients, who were, on that day, five in number. I felt their pulses, looked at their tongues, examined their buboes and carbuncles, made up the medicines which I deemed proper for them, and administered them with my own hands. I determined to continue to do so, and to repeat my visits regularly every two hours. My interpreter, although very much frightened, could not evade accompanying me. He carried the medicines and the sand-glass, by which I reckoned the pulse. I had not only to prescribe,

and to dress sores, but to prepare, compound, and administer remedies, and to register their effects; combining, in my own person, the offices of physician, surgeon, apothecary, clerk, and nurse. I soon perceived that it would be unavailing to attempt to introduce any alteration in respect to regimen, or in the mode of stationing, or attending the patients, or in fact to enforce any regulation, which I could not personally carry into effect. It was not only ignorance, and obstinacy, but determined hostility, against which I had to contend.

Upon my arrival at Constantinople, I found the belief in contagion so inveterate amongst all descriptions of persons, not of the Mahommedan faith, that I deemed it at first expedient to endeavour to conciliate prejudice (but this attempt was soon discovered to be unavailing) by appearing to limit my researches to the consideration of the cure solely. And this seemed to be the more advisable, since, although the prejudices of the Turkish government happen not to coincide with this hypothesis, they have reasons equally cogent for withholding their support from measures, having, for their object, to diminish the devastations of the plague. To this policy, they are not, I think, so much influenced, as has probably upon too slight grounds been advanced by Christian travellers, by the belief that to diminish the

devastations of that malady, is a presumptuous interference with the decrees of fate, as that it is an injurious interference with the particular interests of the Sultan. That speculative notions of fatalism, can ever prevent a whole people from adopting means of self-preservation, is contrary not only to the most ordinary laws, but to the very first law, of nature. The source of this delusion, amongst Christian travellers, in regard to the Turks, may, I think, be readily explained. Christians, not aware of the origin of their own belief in contagion, were unable to account for the disbelief of the Mahommedans, but by attributing it to fatalism. In this matter, they confounded the sentiments and views of the people, with those of the Sultan, and the government. The Sultan, it is notorious, inherits a large proportion of the property of every man that dies. When all the members of a family are carried off by the plague, he inherits the whole of their property. In such cases, the houses of the deceased are shut up, and the keys delivered to his officers; and there are, I have been told, in Constantinople, Smyrna, and the other towns and cities of the Othoman empire, many streets, of which great portions have devolved, in this manner, to the Grand Seignior, and of which many of the houses are empty. The plague of 1813 is said to have filled the public treasury to such a degree, that it

still continues rich. The immediate advantage, which the Sultan thus derives, from the deaths of his subjects, as their heir, appears completely to absorb all considerations of the more remote interest, which he possesses, in their lives, as a monarch. Indeed, this interest, from the uncertain tenure of his power, is but slight.

Nor will this appear unaccountable, when we reflect, that, in a country subject to such frequent, and violent changes of governors, as Turkey, if personal property be insecure, political power is still more precarious; and that, under such circumstances, it must be the first object of a prince, without regarding distant prospects, which it is not probable will ever be realised, to secure a certain portion of wealth for his family, which, in the event of his being himself strangled, or deposed, they may enjoy as a private inheritance.

There is also another motive, which is believed to influence the Turkish government, in not wishing to see the terrors, excited by the ravages of the plague, diminished; and as it is one of a very rational nature, if it were not founded upon considerations so remote, I should be disposed to regard it as of powerful operation. As they do not themselves entertain any dread of this malady, to diminish its devastations cannot appear to them, in a personal view, an object of importance. But it may appear to them an

object of much importance, that the dread, which is now entertained of it, by all denominations of Christians, should not be diminished, but, on the contrary, if possible, increased; since they may reasonably apprehend, that, if, with this dread, the principal objection, amongst persons of that persuasion, to settle in Turkey, were removed, a degree of emigration would take place, from all the less favoured nations of the world, into their fine territory, which might eventually endanger the stability of the Turkish power. In as far as there may be solidity in this speculation, it is curious to contemplate, in the concatenation of political events, that a Pope may have been the most effectual instrument in promoting the permanence of Mahommedan dominion.

The first of these motives, or religious prejudices, as common to all the Turkish nation, cannot, as I have said, be very powerful, when placed in opposition to the desire of self preservation, implanted by the hand of nature in every living being; and I have found Mahommedans as eager for medical advice, and for the restoration of health, as persons of other persuasions¹. The second, or rapacity, appears principally to attach to the prince, and the revenue-officers in his im-

¹ Two Mahommedans, to whom I had an opportunity of administering medical relief, were amongst the most thankful patients I have ever met with.

mediate employ ; and the last, or a regard to the permanence of the Mahommedan power, in so far as so remote a consequence may be an object of consideration, may be presumed to belong to the clergy, the army, and the other privileged classes of the community.

But it was the government only that had any thing to do with my investigation. The personal interests of the Sultan would be directly injured by its success. Every individual, or family, that might be preserved in consequence of my discoveries, would operate a deduction of so much from his usual revenue ; and, if an efficient mode of treatment were generally established, and the delusion of contagion generally effaced, the whole of his income depending upon death from plague, would be annihilated. It was impossible then that they should not desire the failure of my object ; but that object being publicly known, even previous to my arrival, it became necessary for the Porte, either to appear to acquiesce in my views, or openly to oppose them. The latter, although undoubtedly their wish, would be to incur more obloquy than they chose to encounter, in the face of all the ambassadors of the Christian powers ; and indeed they must have conceived it unnecessary to run the risk, since they well knew, not only that no man, who had hitherto entered any of the pest hospitals in Turkey, with similar

views, had obtained any success ; but that, with the then exception of Dr. Valli, none had survived the experiment.

In the representations, which I was desirous of addressing to the Porte, in as far as I thought it expedient to express any opinion, my idea was, that an establishment should be instituted by the Turkish government, according to a plan of my own, for conducting my experiments. But I found, that such a measure was by no means likely to be relished at the Porte. Repugnance to expence was the pretext which it was understood would be assigned ; but the real ground, it was thought, was a determination that the experiment should not succeed. Of the nature of the obstacles generally, I was by no means unaware ; and, when it was first proposed, that one of the Greek hospitals should be the theatre of my operations, it did not fail to occur to me, that the spirit of such institutions must militate directly against the success of any plan tending to promote the preservation of lives ; and that the proffered co-operation could not be founded in sincerity. But I did not, for a moment, hesitate to embrace the project, as the only means of accomplishing my purpose, that were likely to be placed within my reach. From the disease I apprehended nothing ; and I relied, that, if I should be so fortunate as to escape the other dangers, of which I could

not but apprehend the existence, I should be able first to satisfy myself, and afterwards the more enlightened nations of the world, of the correctness of my doctrines, both in respect to the cause, and to the cure, of epidemic diseases.

The task which I had undertaken was, indeed, a difficult one ; but it was rendered the less insurmountable by my being, in a great measure, aware of the obstacles by which my views were likely to be opposed, and all immediate appearance of success to a certainty frustrated. It might be thought, at first sight, reasonable, that the Othoman government should have considered one of the Greek pest hospitals, an institution already formed, as a situation, in all respects the most appropriate for the prosecution of the enquiry which I meditated ; and their readily entering into the project might be deemed a liberal departure from Turkish prejudices. But, on the other hand, it could scarcely be supposed to be unknown to the Sublime Porte, and more especially to Mr. Argiropulo, their principal Dragoon, that the general opinion of the turpitude of these institutions, was very strong, and almost universal : that, for reasons connected with their very nature, the experiments which had been already attempted by physicians, had been followed by no useful results ; and that those who had made such attempts, had almost invariably

fallen a sacrifice to their zeal¹. They could not but have known that the Greek pest houses were designed, not for hospitals for the medical treatment of the sick, but for dépôts, to which the subscribers to the institutions might send their servants, relatives, or dependants, as soon as it was discovered that they were affected with the plague, i. e. when the symptoms had arrived at such a height that the person could no longer conceal his disease, and that it had become already incurable, in order to be from thence duly consigned to the grave, giving to the principals of families, by the removal of their subordinates, what they erroneously regard as their only chance of safety from the malady. It could not have escaped their observation, that, in these establishments, from their very destination, in communities, like the Greek, entertaining a perfect belief in, and the utmost dread of contagion, the executive functions had necessarily devolved to the lowest of mortals, selected, for the most part, from amongst those, who had themselves had the plague; that persons, who, without higher views, can, under such circumstances, consent to exer-

¹ Several physicians, in the Russian service, had fallen sacrifices. Dr. Valli alone escaped. Dr. Whyte perished in Egypt. Mr. Von Rosenfeldt fell a victim after I had finished my experiments. The particulars of each case shall be more fully stated, in the following chapters.

cise functions so repulsive as those incidental to a pest house, will not be content with bare hire, but will remunerate themselves by the spoils of the unfortunate victims, who may be committed to their charge; and that, with a view to the extension and permanence of this inhuman traffic, they will not scruple to make an increase of victims, or to resort to every means, by which they can hope to mar the success, of those, who, with more correct ideas of what is due to their species, may be found endeavouring to dissipate the errors, by which these miscreants live, and thrive.

At the head of these pest establishments, of which the Turks are not so infatuated as to have any, are generally, if not always, to be found, the most ignorant and illiterate, but not the least ambitious, or rapacious, of superstition's sons. Here the race of monks may be said to have instinctively fallen into their native element :

And wild of head to work her fell designs,
Came idiot Superstition, round with ears
Innumerable strewed, ten thousand Monkish forms
With legends ply'd them, and with tenets, meant
To poison reason!

The next officer in command, whom I found at the hospital near the Seven Towers, was entitled Grammatiké. As these, and all the subordinate agents of these institutions, derive their

principal, if not their sole emoluments, from the presents which they receive from the relations of the unfortunate persons, who are consigned to their charge, and from the clothes, and other property, which may be in the possession of their victims when they die, would it not be folly to suppose that they would not desire to increase their gains, or that beings like them would be very scrupulous respecting the means by which they might produce that increase?

When the sick are brought to the hospital, they are attended, if not by friends, or relations, at least by persons interested in their fate, or employed to convey them. Their entrance into the hospital is a matter of great form. The pomp, ceremony, and time, employed in effecting their admission, could not be greater, if the matter in question were a negotiation of state. At the hospital near the Seven Towers, the council, on these occasions, is generally held round a large tomb-stone, under the shade of a walnut tree, near the south door of the building. There it is gravely debated, whether the candidate for admission can be received at all; and, if he can, what situation is to be allotted to him. The scarcity of good places is duly dwelt upon; the crowded state of the hospital fully set forth, and the irksomeness of attendance, in such cases, properly amplified. All difficulties, however,

being got over, and a promise of superior accommodation and attendance being obtained, by means which will readily occur to the reader, the candidate, freed from his suspense, is at length formally ushered into his last abode above ground. There, whilst he lives, his whole fare is rice-water; and, at the very instant of expiration, if not sometimes before, he is transferred to an adjacent burying ground¹. These things are performed by the agency, and conducted under the direction of persons, whose emoluments are in the precise ratio of the degree of mortality arising among those under their care. Their business is not cure, but extreme unction and interment! And, can it, for a moment, be doubted, by any enlightened man, that, under this system, the executive agents of the pest houses, must consider it their clear, distinct, and unequivocal interest, to pray for numerous and speedy deaths; i. e. for the quickest possible succession of customers, with the least possible trouble? RECOVERIES, IF PRACTICABLE, WOULD NOT PAY!

And as there are, amongst such persons, no feelings of a more refined nature, by which the operation of this direct interest can be counteracted, may not its consequences be rationally presumed to extend beyond the destruction of in-

¹ Dr. Valli, who was also in the hospital of the Seven Towers, states the same fact.

dividual pestiferous patients, even to the strangulation of discovery itself? Sensible, that, if the plague were shewn not to depend upon contagion, or an efficient method of cure established, their influence and their occupations would fall together; is it to be imagined, that, in opposition to such a project as mine, the endeavours of these executive agents would be strictly limited to the ineffectual formality of speculative wishes?

These observations will apply alike to all institutions similarly founded. To expect that persons, who exist, or thrive by the death, or burial, or spoils, of their fellow-creatures, should generally concur, or rejoice in the discovery of additional means for the preservation of lives, would be to expect impossibilities. Hence, those rare instances of disinterestedness, which constitute exceptions, reflecting honour upon human nature, as Fra Louigi di Pavia, of Smyrna, celebrated by Howard, are deservedly regarded with veneration by mankind.

Such is the nature, and complexion of the establishment, in which I now found myself placed. But I was not taken by surprise; for scarcely any thing which I here met with, exceeded in turpitude the expectations, which, from general reasoning, I had previously been led to form'. I may truly say, that I entertained no apprehension

¹ This applies to the servants only.

of danger from the plague. The conviction which I felt of the non-existence of contagion, diminished, in my mind, the chances of my being affected; and, although I was aware, that the intrinsic severity, and danger of maladies, are not altered by the nature of their cause; and, that as a stranger to the climate, and in the usual season of pestilence, and in such a situation as that in which I was placed, whether in respect to the moral or physical causes, by which I was liable to be acted upon, there was at least an equal chance of my being affected with the malady, I had sufficient reliance upon my experience of the cure of acute diseases, not to allow my mind to be disturbed by the considerations, which might otherwise have unfavourably affected it. The desertion of my servant I now found to be in many ways a most serious evil; and more especially, as it left me almost wholly at the mercy of an interpreter, whom, if he had even been otherwise qualified for the office, I discovered to be a troublesome fanatic, whose conduct soon afforded no unequivocal presumption that he was employed rather to watch, and to frustrate, than to conduct, and to aid me. I had also committed the egregious imprudence of being without arms. But the consideration was not now, what might have been better done, or better omitted; but how to act

most effectually, with the means which were in my power.

It will naturally occur to the reader, to enquire, upon what grounds, under such circumstances, I could possibly rely upon any degree, or chance, of personal safety? I will state, as accurately as in my power, the impressions upon my mind. From the fate of my predecessors, and the prevalent traditions respecting their proceedings, there could be no rational ground for doubting, that, if the progress of my investigation should not be arrested by the intervention of disease, the disposition to frustrate my object, by other means, existed in full vigour. But there were strong reasons, why the means usually resorted to, on such occasions, could not safely be acted upon. The object of my voyage having been officially notified to Sir Robert Liston, by Lord Castlereagh, in April, my intentions were known for some months previous to my arrival, and my views consequently matter of notoriety, at least amongst the diplomatic body. My experiments were commenced with the sanction, and apparently the support of the Turkish government. Messengers, and notes were frequently passing between myself, whilst in the hospital, and my friends at Pera. The notoriously infamous character of these institutions, rendered it too hazardous for them, in the face of

all the ambassadors of the Christian States, to attempt a *coup de main*, either by means of poison, or strangulation, which, from their suddenness, and violence, could not admit of being confounded with the effects even of a severe, and sometimes rapid disease, like the plague. The hospital, besides pestilential patients, contained a number of poor persons, of both sexes, in good health, admitted from charity, and was consequently well inhabited. These persons had no motives to connivance; and they were too numerous to render secrecy practicable. The execution of such a design, and its responsibility, must have entirely rested with the servants of the hospital, who must have been well assured, that, even if they were to act under the orders of the Directors, and the Porte, they would, in case of detection, be disavowed, and delivered up to punishment. An attack of the plague, accompanied by delirium, could alone give them the desired opportunity. And, if such a coincidence had occurred, I have no doubt, that, independently of the principle, that, persons who live by error, are the natural enemies of discovery, my watch, clothes, and money, would have been quite sufficient inducements to have led to the perpetration of a deed, which would have saved the reader the trouble of perusing these pages. But I am not, by any means, of opinion, that

they would not have attempted to strike a blow, even at the risk of detection, had they not considered it more expedient to wait, in the confidence, derived from experience, that I should, in process of time, be seized with the disease, and either succumb under the force of the malady, or by falling into delirium, afford them an easy opportunity of completing the catastrophe, without the risk of detection. In stating the case of Mr. Von Rosenfeldt, these points shall receive farther elucidation.

I depended, then, for my preservation, upon the probability of my being able, by my knowledge of the cure of acute diseases, of rendering any attack of the plague in itself but little dangerous, and of wholly repelling delirium; that I should, by the resources of my mind, exerted under these circumstances, to their highest tone, be enabled to frustrate the machinations of the miscreants, by whom I was surrounded: and, for the rest, I relied much, in such a cause, upon the protecting shield of an all-discerning Providence.

As fame can be of no consequence to the dealers in plague, it cannot be presumed that the dread of exposure could operate with the mere executive servants of the hospital, as a motive to frustrate my investigation, or to destroy myself, farther than they might suppose such exposure

to tend to the ruin of their trade. With the Directors, however, and still more with the Sublime Porte, such considerations could not but be of some weight. And I shall endeavour so to act, that, in this view, my death would have been no ordinary advantage to them: for, since they have failed to add me to the number of their victims,

————— it shall go hard,
But I will delve one yard below their mines,
And blow them at the moon!

HAMLET.

CHAP. XXV.

Death and admission of patients—superior accommodation of the Papa and Grammatiké—the servants drag the sick in their arms with impunity—an idiot sleeps in contact with pestiferous patients without detriment—persons in health occupy apartments promiscuously with persons ill of the plague, and are not seized with the malady—letters from Sir Robert Liston, Mr. Morier, and Mr. Pisani—flattering but deceitful appearances—proposed deputation from the apothecaries of Pera—the Papa Parthenius suspected of accelerating the death of patients, and dismissed—means resorted to by the executive agents, in order to prevent my success—an intermediate Papa—the Papa Nioffité.

WEDNESDAY 16th to Friday 18th August.—Continued to attend the sick, as before stated. Two patients have died, and two new patients have been admitted, since the last report. I observe that the apartments occupied by the Papa and Grammatiké are situated on the south front of the building, facing the Propontus, that they are well white-washed, and the windows well sashed and glazed; and that they are, in all other respects, very comfortable, whilst every other part of the house is in the most neglected condition. Their mode of living is, no doubt,

correspondent. To see these persons dragging pestiferous patients in their arms, (which they did at my desire without any reluctance) placing them in bed, and handling them in every manner, it is scarcely in human credulity to believe, that they themselves can consider the disease contagious, however profitable they may deem it to impress the belief upon others. Conscious that they have not the means of cure in their power, they would never, were they not convinced, from experience and observation, that the plague cannot be propagated by means of contact, venture to drag the patients in their arms. It is not, for a moment, to be credited, that, in a disease almost certainly destructive, even the most overweening desire of gain could induce persons to subject themselves constantly to so inevitable a danger, as contagion would infer. And as they cannot be supposed to be actuated by motives of a higher nature, in encountering so great a peril, all these contradictions the advocates of contagion will be under the necessity of endeavouring to reconcile, by supposing various degrees of susceptibility; of which, in order to be consistent, they must also suppose Papas and Grammatikés to be, at all times, wholly destitute. The Papa has had the disease once, and the Grammatiké declares that he has had it four times; but not in their present situation, although they have been for many years,

in the service of the hospital, every autumn in contact with pestiferous patients, and the plague being notoriously capable of affecting the same person repeatedly. Not only do the ordinary servants of the hospital, who are almost constantly employed about the sick, turning or raising them in bed, when they have lost the power of motion, dressing their buboes, &c. remain in health ; but persons, labouring under other diseases, and persons in health, admitted out of charity, when unable to procure the means of subsistence, sleep in perfect security, in the same apartments with those afflicted with the plague. It was not unusual for four or five, or even more persons, of various descriptions, some with plague, some with other diseases, and some with none, to inhabit the same room, without experiencing or apprehending any mischief from the intercourse. This practice was adopted for the convenience of the executive agents, who could thus administer to their wants with less trouble to themselves.

The barber of the hospital used to sleep in the same room with Cristophe of Cephalonia, who was ill of the plague ; and many of the poor persons in health, both male and female, ventured to accompany me, at different times, in my rounds among the patients ; and, I make no doubt, would have handled them without the least dread. The interpreter, who constantly accompanied me

amongst the patients, without any precautions that I know of; eat with me; walked with me; and was of course frequently in contact with me, even after I had been seized with the plague, was not affected with the disease. He had never had the malady. He is in the prime of life, of a vigorous constitution, and inured to the climate.

I observed that it was customary for an idiotical person, who was permitted to inhabit the hospital, in the quality of a hewer of wood, and a drawer of water—a sort of Caliban—to lay himself down to sleep, with his head and neck on the bed (their beds are on the floor) of Demetrius, of Roumelia, a pestiferous patient, who died on the thirty-ninth day of the disease, and generally in contact with some part of his body. Upon enquiring into the matter, I was informed, that, having no bed of his own, it was his constant practice to sleep with some one of the pestiferous patients, and that he has never been known to suffer detriment from it. He is said to have had the plague once; and not to have been always idiotical, the debility of his mind having been occasioned, according to his own account, by a potion, administered to him by a woman, to whom he had been attached, at Constantinople.

This day, sent a Report of my proceedings to Sir Robert Liston.

Saturday, August 19.—Received the following answer:

British Palace, Pera, 19th August, 1815.

Dear Sir,

Your trunk was safely deposited in this house, and will be taken care of.

I thank you for your *First Report* of your proceedings, and hope you will continue to give an account of yourself. It will be read with very great interest, though of course you must not expect regular answers.

You have not made me acquainted with your theory respecting the plague, nor with your plan of treatment; but the conjecture I make on the subject, from what has dropped from you in conversation, induces me to make one request.

Your persuasion seems to be, that the disease is *not contagious*, and consequently that there is no need of taking precautions against it.

But as we are all fallible mortals, and as your conviction does not arise from personal experience or experiment, there must be a *possibility* that you are mistaken; and therefore, looking to the extensive benefit that may arise from the success of your efforts to cure the distemper, I do not think it unreasonable to propose that you should take the simple method of preventing infection from the touch, *until* such time as *you have cured not two or three patients, but* a number sufficient perfectly to establish the efficacy of your method.

When you have done this, I will not importune you with farther remonstrances. As an individual, I have no right to interfere with your practice, or your conduct ; but as a *public man*, addressing myself to a man employing himself with a view to a great *public good*, you will perhaps admit my claim to speak.

I beg you will believe me to be, with perfect truth and regard,

My dear Sir,

Your most faithful humble servant,

ROB. LISTON.

P. S. Mrs. Liston joins me in best compliments and good wishes.

With the highest deference for Sir Robert Liston, both in his public and individual capacity, and with every possible inclination to comply with this his considerate and friendly request, I found it, however, impossible to do so, without entirely relinquishing my object. It was only by administering the remedies with my own hands, that I could have had any perfect assurance of their being applied : for, even in my very sight, the attendants would have endeavoured to deceive me ; and, to have administered the remedies without contact, would have been impracticable. But,

independent of this, contact was altogether indispensable, in order to enable me to form a correct judgment, respecting the state of the pulse, which, in such maladies, together with that of the tongue, affords the only certain criterion of the danger and intensity of the disease. Indeed the experimentum crucis, upon which I had determined, admitted of no half measures. And they would have been rendered the more unpardonable in me, that my full confidence, not built upon vague hypotheses, but derived from induction of experiment, of being able to cure the disease in my own person, if it should happen to assail me, as well as in other patients, enabled me to meet the danger with a fearlessness and security, which persons otherwise circumstanced, could not be supposed to feel.

The communications of Mr. Pisani, throughout this investigation, as they may be considered official, properly form a part of the narrative of my transactions. One, which I received from him, of this date, is in these words :

Pera, 19th August, 1815.

My dear Sir,

I received, with great pleasure, your three letters, by the return of my last messenger ; and was truly glad to find you are in good health and good spirits.

His Excellency, Mr. Liston, and Mr. Black, for whom two of the three letters were, expressed great satisfaction at hearing from you, and herewith you have an answer from his Excellency, who, as well as every friend of humanity, takes a hearty interest in your undertaking.

Please to inform me if you are properly attended upon: if your accommodations are satisfactory; and if the horse is in the stable. I remain ever at your command,

My dear Sir,

Your devoted humble Servant,

B. PISANI.

P.S. I inclose a letter from Mr. Morier.

The horse, like every thing else that was supplied by the directors of the Pest Hospital, was furnished with great reluctance, after much delay, and of a bad quality. With good horses, riding would, under the circumstances in which I was placed, have been an appropriate recreation. But the miserable rip which I selected, out of half a dozen Rozinantes, that were sent to me to choose from, I did not cross above two or three times: and even the interpreter, except upon one occasion, declined the offers which I made him, of the use of this steed. The accommodation, and the

attendance, were all execrably bad ; and the paucity of subsistence amounted to absolute want. But, in my representations, I considered it expedient, to do no more than to glance at the truth, leaving the extreme degree of the inconveniences to which I was exposed, to be inferred by my correspondents : for, I was apprehensive that a full delineation of them, without effecting an amendment, might tend to mar the main objects of my pursuit.

The letter, from Mr. Morier, which I received along with the preceding communication, was of the most kind and friendly nature ; but, as it was entirely private, it could not tend to the elucidation of my subject.

It was now the fifth day since I had come into the hospital. Every thing appeared to be going on most prosperously. The Porte affected to be quite delighted with my proceedings. Some were astonished, some surprised, some incredulous, some vexed and some pleased, at the complexion of the reports, which were in circulation. The fame of my exploits was duly reflected back to me by messengers from Pera, and from Constantinople, with such inaccuracies, or exaggerations, as it had gathered in its course. Perhaps I was myself the only person, who was not deluded into the premature expectation of an immediate triumph ; because I alone could know the precise

state of the case. Some of the apothecaries of Pera, hearing that I was vanquishing the plague *au pas de charge*, and expecting that I should, in consequence, have a great demand for medicines, proposed to send a deputation to wait upon me; and requested to know when it would be agreeable to me to receive them. Appreciating the motive, and knowing that I had opponents more formidable than the plague to vanquish, before I could achieve an acknowledged triumph, I declined giving an immediate answer.

The very favourable expectations, which had thus been raised, no doubt accelerated the period of re-action. The trade of the plague agents was in danger: and they determined, at any rate, to save their trade. From a great and unexpected difference between the situation in which they were left at night, and that in which they were found in the morning, there was too much reason to infer that the deaths of the two first patients, who died, Vimach, of Aptivisu, and Constantine, of Roumelia, were at least hastened by violent and unjustifiable means. With respect to Vimach, when I first saw him, at six o'clock, P.M. on the 15th, although he was very ill, he was by no means in such a state, as to warrant any expectation of his death the following morning. He died at half past eight o'clock. But no one could be doing apparently better than Constantine, on

the evening of the 17th. He had taken four doses of his medicines, in the course of the day; and, at ten o'clock, at taking the fifth dose, he continued to do well. His pulse was only ninety-six, skin moist, and tongue cleaner. How great, then, was my surprise, to find him, in the morning, with his pulse at one hundred and thirty, tongue black, hard, and dry, and dissolution approaching. Still, although I was both mortified and disappointed, and thought it very strange, I continued, throughout the morning, to attribute these deaths to the inveterate nature of the disease; and it was not until other circumstances, calculated to give rise to strong suspicions, had transpired, that I could allow myself to entertain them. This inference was supplied by the conduct of the Papa, in the evening, respecting another patient, against whom he appeared to have entertained similar designs. He took occasion to inform me, as we were going our rounds, that Tumus, of Roumelia, the patient in question, who was admitted on the 17th, covered with petechiæ, and labouring under a most severe malady, had got out of bed that afternoon, and had been found running about the yard in a state of delirium. It fortunately so happened, that I had, only a few minutes before, visited Tumus, a circumstance of which the Papa was ignorant;

and, having left him, in respect to all his functions, approaching the state of convalescence, I did not hesitate to declare, that I wholly disbelieved the statement. "But we shall see in the morning," I added, "whether Tumus is to share the fate of the two unfortunate men who have already perished." This remark, accompanied by the intimation, that I meant, in the morning, to make known his conduct to the directors of the hospital, appeared to have had the effect of deterring the Papa, for the night, from his projected tragedy : for Tumus continued to do well, and soon recovered'. Early this morning, I walked into Constantinople, accompanied by the interpreter, in order to inform the directors of my suspicions. Before we set out, the Papa appeared very anxious to induce us to drink coffee, which however I declined ; as did the interpreter ; and we breakfasted in one of the

* Whilst this patient was in a state of convalescence, they contrived to send him away from the hospital ; and *afterwards* informed me that he was gone to Constantinople. The moment a patient got convalescent, and able to eat, he would naturally desire to leave a place, in which he could get no food. But there was a farther motive on the part of the servants of the depôt, to hasten the departure of those who were able to go, since they might, with them, swell the number of nominal recoveries in the other hospitals ; and thus detract from my proceedings by a false comparison.

coffee-houses of the city'. We found three of the directors; and I desired Constantine to acquaint them, that, it was my determination, unless they instantly dismissed the Papa, forthwith to quit the hospital. They apparently complied with my request. Parthenius (that was the name of the Papa,) immediately disappeared. But I afterwards heard, that he had only been sent to the hospital at Pera, and the Papa of the hospital at Pera, transferred to that of the Seven Towers. This was a mild, good looking young man, of benevolent aspect, and modest demeanour, apparently very unfit for the succession. I observed him frequently sitting on the beds of those pestiferous patients, who were in the most danger, and in contact with them, occupied in writing. What he was committing to paper, I did not learn. Being probably found unfit for their purpose, he only remained two or three days; and was succeeded by one of the most profligate

¹ On this, as on some other occasions, when he walked out along with me, this fanatic used to run towards every Greek Church, which came in our view, however distant; and I had to wait until he had duly sprinkled himself with holy water, crossed and prostrated himself before ever saint or sinner, who happened to have a niche in the interior of these buildings. Finding that I must wait, I generally chose to accompany him; and thus had an opportunity of observing that his worship of graven images was certainly most animated and sincere.

rascals I have ever met with, in any situation. This last person was constantly engaged in smoking, or drinking; was regularly intoxicated in the evening, and always disturbing the patients with riotous behaviour. His name was Nioffité. That of the intermediaie Papa I omitted to note.

When, upon remonstrating with the directors of the hospital, against this successive appointment of Papas, I found that they considered a Priest a much more essential part of the establishment, than a Physician, it was utterly impossible, any longer, not to despair of this institution, as to all purposes of medical investigation.

The executive agents of the pest depôts in the Levant, consider all investigators into that malady as their natural enemies; and the Papa is generally the chief of these agents. The mode of opposing enquiry may vary according to the character of the individuals; but the principle of their conduct must be always the same. The plan of the campaign, against the common enemy, was, no doubt, laid, before my entrance into the hospital; and, although it might be deranged, or interrupted, I never formed the idea, that, by the removal of Parthenius, it could be destroyed. I was aware, that, whatever Papa succeeded, the system would remain

the same. But, I also knew, that, if even they could contrive, in the mean time, to make it appear that all the patients had died under my hands, and in consequence of my treatment, I should ultimately be able, if I should be so fortunate as to escape, to prove the correctness of my doctrines to the world. This will require nothing more, than a repetition, which I am ready to make, of the experiments, by which my results were at first obtained.

The following were some of the principal means resorted to, in order to prevent my appearing to be successful. The town was ransacked, to find out every lost case of plague, that existed, in order to send them to the hospital of the Seven Towers, whilst all the mild, or incipient cases, were sent to the others. It happened, by one of those blunders, by which low cunning usually betrays itself, that a boy, in the incipient stage of a slight malady, was, one day, instead of being sent to Pera, brought to the hospital of the Seven Towers, whilst I was reposing. Being strong and able to walk, he was conducted into my chamber; and I directed the servants where to place him. In this case, from the appearance of the patient, I flattered myself with an easy cure. But, before I was ready to prescribe for him, and to visit the other patients, after having reposed, the inter-

preter came to inform me, that the boy, disliking the hospital, had already run away. This I knew to be impossible, without connivance : and I told him I believed it to be a villainous trick. Perceiving, however, that it was part of the system ; and, knowing that there was no remedy, I determined not to suffer myself to be vexed.

Another part of the system was to endeavour to dissuade the patients from taking their medicines ; in which, had I not administered them with my own hands, the Papa, by virtue of his spiritual authority, might perhaps have been able often to succeed. But, as, in severe diseases, not many patients will be persuaded to renounce the chance of recovery, which the administration of remedies offers ; and, as my presence was sufficient generally to counteract this influence ; the intrigue proved successful only in one instance, that of Nicolai, of Carfania. In the instance alluded to, the patient, after having become convalescent, positively refused to take medicine ; and this refusal was distinctly traced to the source I have mentioned. The interpreter declared it, and the patient himself acknowledged his determination to have been instigated by the Papa ; but still persisted in his refusal. Full proof of the existence of this system was afterwards developed, by the false reports, which the plague executive agents put in circulation at Constan-

tinople and Pera, after my departure from the hospital, respecting the comparative mortality at the different depôts, and which must have formed the foundation of a paragraph of Sir Robert Liston's Letter to Lord Castlereagh, as well as the *pretext* of the Porte for declining to institute the establishment, which I proposed, after my recovery from the plague.

These reports, if not necessarily credited by the public, at least cannot be refuted by them. Indeed they have no means of ascertaining the truth, excepting by comparing and contrasting the accounts of the plague agents with each other; and, besides that it is the interest of these persons to agree, in their narratives against the common enemy, it is impossible, where no public discussion prevails, to arrive at the precise truth, in matters much more accessible to common observation.

Apprehensive that these minor modes of counteraction might not have the effect of preventing my appearing to be successful, the executive agents of the plague depôt, seeing the vigor of my measures, became, from the first day, seriously alarmed; and, in order to ensure the frustration of my object, determined, as I have already stated, to resort to the most efficient operations. One of the consequences, rendered indispensable by these operations, was to bury

the dead, before I should be informed of their decease. This they regularly put in practice¹. Hence, had I even been able to undergo the additional fatigue of dissection, or had I had assistance of any kind, no examination of the bodies could have taken place. This, to be sure, might, after four or five days, have been remedied. After I should have written to Mr. Pisani; and after he should have applied to Mr. Argiropulo, the first dragoman of the Porte; and after Mr. Argiropulo should have received his instructions from the Porte, and issued his own to the directors of the hospital; and after the directors should have issued their orders to the executive agents, presuming all the persons concerned to be acting with sincerity, the practice of interring the bodies of the dead without my knowledge might have been discontinued. But the regulation would have come too late.

¹ A fact to the same purpose is related in Dr. Valli's Journal.

CHAP. XXVI.

Am seized with the plague—embarrassment of my situation—mode of proceeding—conceal my malady—continue to visit the patients—first symptom, vomiting—second day, buboes—a visit from Mr. Pisani—desire to be removed from the hospital—convalescents discharged—communications from Mr. Black and Mr. Pisani—observations on the diet with which I was supplied—disclose my illness to the interpreter—effects of the disclosure—spirit of the Greek Priesthood—write to Mr. Pisani—his answer—meditated plan for my destruction—visit from two Armenians—strong message by one of them to Mr. Pisani, &c.

SUNDAY, August 20th. Whilst I was deliberating on the expediency of making any remonstrance upon this subject, the question was reduced into a narrower compass, by my being myself seized with the malady; rendering the means of self-preservation my most immediate duty. These means being immediate removal from the hospital, it was obvious, that, to make any stir in a matter, which would have required several days to accomplish, would have been fruitless.

I was now in a situation sufficiently embarrassing. Seized with a malady, in nine cases

out of ten, fatal ; abandoned by my only personal attendant ; surrounded by persons, not simply upon whom I could have no reliance, but positively interested in my destruction, and not deemed remarkably scrupulous about the means ; and requiring, for my removal, a negociation at least as long as is usually necessary to decide the fate of a pestiferous patient ; the question, what ought to be done, was not of easy solution.

The manner in which I solved it was diametrically opposite to that, which, at the first view, might appear to be the most eligible, and which would probably have been chosen by nine hundred and ninety-nine persons, out of a thousand. Instead of declaring my situation to my official correspondent, Mr. Pisani, and requiring to be immediately removed, I cautiously concealed the circumstance, for some days, from all the persons in the hospital. The motives of my conduct were these. I felt assured, that, before the negotiations necessary for my removal could be completed, I should either have vanquished the disease, or the disease would have vanquished me. Had I been under the protection of a Turkish community, and understood the Turkish language, there could have been no difficulty. I might have gone from the hospital to any other place of residence, without ceremony, or delay. But belonging to a Christian community, the case

was different. Even without disease, from having been amongst it, quarantine would have been reckoned necessary; but, being affected with the malady, it was deemed doubly indispensable. Under these circumstances, I thought that any premature declaration, upon the subject, could only produce mischief, by allowing the servants of the hospital to know, earlier than was necessary, how much I was in their power. I therefore continued to visit the patients, as before, until my final departure, excepting on Monday, the 21st of August, being the second day of my illness, on which day, I found myself constrained to remain in bed, and part of the 23d, on which I was only able to pay partial visits. On both days, I sent medicines to the patients; but without any reliance upon their being administered.

The disease commenced on Sunday afternoon, with vomiting, which seized me suddenly, upon being awoke, in order to visit a patient. The vomiting was momentary, and never afterwards returned; nor did I meet with that symptom in any other case, during my residence at the hospital.

Monday, August 21st. Early this morning, upon passing my fingers over the top of my left thigh, in consequence of experiencing a sensation of stiffness, I discovered the usual swellings in the inguinal glands; and the same thing immediately afterwards in the right groin. I was now assured of the existence of the plague.

In the course of the day, I received, from Mr. Black, of Pera, a supply of wines, bottled porter, &c., which proved of great service. If I had used the precaution to have taken them along with me, it would probably have been a mean of obviating, or at least retarding, the accession of the disease. But I had unwisely relied upon the profuse professions of the directors of the hospital, that nothing should be wanting, that I might require.

Tuesday, August 22nd. Had a visit from Mr. Pisani, who came on horseback. Our meeting was in the burying-ground, where I happened to be at his arrival. He took his station to windward, and at a considerable distance. The interpreter being present, I did not choose, for the reasons already stated, to enter into any explanation respecting my illness. Although they might suspect it at the hospital, and it might be rumoured at the Porte, and Mr. Pisani seemed to me to be aware of it, yet I thought it better to let it still remain a matter of uncertainty and conjecture. I, however, intimated (which I thought would be enough to a man of Mr. Pisani's experience and penetration) my wish to be immediately removed from the hospital, and to have quarantine quarters provided for me at Pera, or elsewhere.

After a week's remonstrance, persons now arrived to glaze the windows of the hospital;

but the damage, in as far as my health was concerned, was already done.

Wednesday, August 23d. Was able to visit the patients only partially. Part of the day confined to bed. Christophe, Nicolai, and Tumus, have left the hospital in an advanced stage of convalescence. Received a friendly letter, of this date, with parcels, from Mr. Black, which were very acceptable. Received a letter also of the same date, from Mr. Pisani, in which he says :

“ I send Dr. Macguffog’s answer to your message, through me, of yesterday, with two parcels, from Mr. Black, which I hope will be acceptable. Before I proceed to look out for a lodging on this side of the town, I wish to have your farther instructions; and particularly to know what I am to say when the Porte comes to know of the circumstance. Every one at the palace was happy to hear of your being well, and I was truly rejoiced, indeed, in finding you *more hearty than I expected*. I am just setting out for Buyukderé, *to be back on Friday morning*.”

Very comfortable news for a man in the height of a plague, (for Monday the 21st, and Wednesday the 23rd were my two worst days,) to be informed that he must remain at least three days longer, the period in which a man’s fate is usually decided, under the roof with persons, every one of whom was ardently looking out for his death ! Indignation, we are told, makes verses ; and I

found that indignation gives strength. I got out of bed, put my clothes on, and walked forth to ruminate in the burying ground. The reader will recollect that Mr. Pisani was my only medium of business, and of intercourse; and, that, without him, I could get no lodging to hire, nor any means of removing from the hospital. In this state of things, he set off for his country residence at Buyukderé, twelve miles from Constantinople, on Wednesday. He was not to return till Friday, and it would be impossible to remove before Saturday. Here then was I fixed, for at least four days; sufficient, if I had got worse, to be sacrificed four times over, if I had as many lives. I was by no means satisfied with the proceeding, or the reasons for it. What cared I what the Porte might be pleased to say, or to think upon the subject. I was not one of their slaves. I was not receiving; but conferring favours. I had not engaged to remain there for any specific time; and, independent of the consideration of sickness, it was quite sufficient that it was my wish to be removed. With respect to farther instructions, what instructions could have been necessary? There must surely have been abundance of lodgings; and a few piastres less or more, in such a case, could not have been an object

— in the way of bargain; mark ye me,

It was no time to cavil on the ninth part of a hair.

HENRY IV.

There were funds of mine in Mr. Barbaux's hands, for immediate application. Without meaning any imputation against Mr. Pisani, whose situation, as well as my own, I am aware, must have been exceedingly perplexing, it was impossible to avoid making these reflections. And, having done so, all that remained for me farther to accomplish, was to avoid dying of the disease, or by strangulation, or poison, till Saturday.

Thursday, August 24th. This morning I received a civil message from Mr. Stavrack, treasurer, and one of the directors of the hospital, with a shoulder of mutton. In return, I sent him a statement of pestiferous patients, as follows: Three dismissed cured:—Three dead:—Five in the hospital, expected to recover. The incident of the shoulder of mutton led me to make some remarks upon diet. It was now one of their seasons of fasting, and they did not seem to question that fasting would be salutary for me as well as for them. In Lent, their principal dish seems to be *caviere*. They had also a few other dishes, of a similar description; but all so immersed in villainous sauces, as to be inaccessible to civilized stomachs. Their fish, when there happened to be any, was rendered useless by the same means. I could never, by any entreaties, prevail upon them to get me a fowl, or any kind of meat, plain boiled, or roasted; nor, indeed, to depart, in the smallest degree, from their own taste,

whether in respect to cookery, or the kind of food to be provided. To give some instances of the manner in which they thought fit to conduct the affairs of the table : I expected, of course, that the joint of meat which had been shewn to me this morning, would have been served up for dinner ; but it never afterwards made its appearance, in any shape ; nor could I ascertain how it had been disposed of. At another time, a boiled fowl was produced, and snatched away in a similar unaccountable manner. Rice and milk having, by some accident, been, two or three times, produced, I tasted of it, in the absence of less insipid food, requesting, at the same time, that it might be continued. From that moment, it was never presented. Upon the whole, their plan of privation possessed a method and perseverance, which could not have been the result of mere ignorance and obstinacy. Thus, I had no alternative, but to extract nourishment out of some bad fruit ; and, to add to the mischief, I was, for about a week, wholly destitute of the liquors to which I had been accustomed. Thus, by the effects of privation, on the one hand, and exposure to currents of air, from the dilapidated state of the building, on the other, my constitution had been considerably impaired, during the first five days of my residence at the hospital, and I became an easy prey to disease. I think it right to be particular in detailing these circumstances, for the

information of others ; assuring them that they will find no means so powerfully anti-pestiferous as good living, and guarding against the undue influence of the weather. In this latter view, the buildings in Constantinople, and throughout the Turkish dominions in general, are of a very unfavourable construction. They are too much in the nature of sieves. And, in the production of disease, I am persuaded that much more ought to be attributed to this circumstance than is generally imagined. But this is not the proper place for entering into such a discussion.

Friday, August 25th. I considered myself much better, and indeed out of danger, although my tongue was still extremely foul, and taste bitter. I tried, for the first time since my illness, to smoke. Mr. Pisani had kindly provided me with a Turkish pipe ; and previous to my indisposition, I used occasionally to find some amusement in smoking from it. Upon the strength of these favourable appearances, and expecting immediately to be removed, I thought I might venture to declare to the interpreter the nature of my malady, shewing him, at the same time, the glandular swellings, by which it is generally accompanied. He received the intelligence with a ludicrous mixture of horror and exultation : for, although he was himself a daily, and a standing proof of the non-contagious nature of the disease, I have every reason to believe that he considered

it highly sinful in me to assert that doctrine. He appeared to have thought the malady a judgment upon me for my disbelief, and sagaciously concluded our *entertien* with his usual exclamation of "Dieu fait tout." I was glad to find that the information had the desired effect, of making him keep at a greater distance. My principal reason for making the inmates of the hospital at all acquainted, formally, with my illness, was the persuasion, that, if they should find it afterwards expedient, they would not scruple to assert that I never had the disease, unless I had put it in this manner beyond a doubt.

Last night I fell asleep in the nitric bath, and did not awake till the water began to feel cold.

Saturday, August 26th. I may here mention an incident illustrative of the spirit of the Greek Priesthood. Having occasion to send a quieting medicine to a young girl, who was in excruciating pain, but past recovery, with directions that it should be immediately taken, the Papa Nioffité sent me a message that he was then going to give her absolution, which must be done *first*; but that *afterwards* the medicine should be administered according to my directions.

Last night took three grains of opium. Used brandy and water for drink, which I have continued during the day, finding the cold tea, which I have hitherto, almost from the commencement

of my illness, alternated with it, now become insipid, or disagreeable.

Still heard nothing from Mr. Pisani.

Sunday, August 27th. Last night I used the nitric bath at nine. At eleven, took between three and four grains of opium, and at one o'clock the same quantity. Used principally brandy and water for drink, but occasionally cold tea, and bottled porter, according as my taste, or fancy prompted. I was calm, but slept little. Skin moist, pulse and tongue as usual. Mind wandering occasionally, but only in a slight degree. Have thought the laudanum conjoined with the tincture of litta, and antimonial wine, more appropriate than the solid opium alone, in regulating perspiration, and composing to sleep.

Still no tidings of Mr. Pisani, or of quarantine quarters. The delay I considered extraordinary. Officially announced to him my illness, and my being out of danger from the disease; but urging my being removed from the hospital. Received an answer as follows:

Pera, 27th August, 1815.

My dear Sir;

“The main object of sending the present messenger is to express the high and true satisfaction of all your friends here at your recovery from the malady you have got at the Greek Hospital; it having proved to every one of us peculiarly

grateful to have learnt, at the same time, your illness and recovery from that cruel disorder.

“ I have next to inform you that I have very punctually communicated the contents of both your letters to Mr. Liston, who desires his kindest compliments, and intends to propose to Dr. Macguffog, who dines with his Excellency to-day, to go to see, and converse with you to-morrow. On his return we will be more minutely informed of your wishes, and be better able to offer you any farther assistance you may want.

“ In the mean time, go quietly on, and keep up your spirits as you have done hitherto, to the admiration and even surprise of every one.”

This was doubtless excellent advice. But—

Who can hold a fire in his hand,
By thinking of the frosty Caucasus ?
Or cloy the hungry edge of appetite,
By bare imagination of a feast ?

Monday, August 28th. Last night, as I was preparing to go to rest, my attention was attracted by an unusual bustle, and the sound of several voices in the lobby, near my chamber door, where we used to dine. Suddenly opening the door, I perceived the Interpreter, the Papa, the Grammatiké, and another, in close and serious consultation. At seeing me they appeared confused. The circumstance was quite unusual, or rather had never happened before ; for it was

considerably past their ordinary bed-time, which they were generally very punctual in observing. I ordered them to retire, and to make no noise. But, as I scarcely expected to be obeyed, or rather indeed expected that the consultation might have some serious result, I endeavoured to make them sensible, by instantly shutting and securing the door, that I was prepared to dispute the entrance. I relied, that, in case of an actual attempt to enter my chamber by force, I could, by such means as were within my reach, make a resistance sufficient to attract the attention of the inhabitants of the hospital generally, and to bring them to the scene of action. Whether the conspirators were apprehensive of this, or might have thought that I had fire-arms concealed in my portmanteau, or that the matter was not yet ripe for consummation, I know not : but, in a few minutes, they retired ; and I heard no more of them.

To apprehensions of this kind I wholly attributed their forbearance, being as much persuaded, as it is possible, in such a case, to be, that it was intended, that night, if a favourable opportunity should occur, to destroy me. Combining this proceeding, with the report of my death, which was circulated, the following day, it will not be deemed unreasonable to presume that such a plan had been actually formed ; and, that, in the confidence of success, which, how-

ever, my vigilance, and the improving state of my health, happily enabled me to frustrate, the report of my death, which had not even for foundation the occurrence of a relapse, or of any aggravation of the malady, might have been unguardedly and prematurely circulated.

The interpreter I regarded as the greatest culprit, because, besides being a conspirator, having been originally officiating in a confidential capacity, he was also a traitor. He had previously repeatedly manifested symptoms of disaffection, and insubordination, approaching to open mutiny; but which appeared clearly to be the result of a deliberate plan, rather than of any casual dissatisfaction, or ebullitions of temper. He had, indeed, no cause of dissatisfaction; having every thing in common with myself, and being allowed the free use of my wines, bottled porter, tea, &c. to all of which, especially to my brandy, he was very faithfully devoted.

He was frequently engaged in writing notes to the directors of the hospital, which I supposed to consist, not of reports of what was literally passing (for much of what he knew it was his business to suppress, and of all that related to medicine he could know nothing), but of what it suited their system, or rather the system of the Porte, of which they are but the tools and slaves, should be propagated. But, respecting this, I

was perfectly indifferent: for, however the false, *ex parte*, or ignorant representations of this person, might serve the present purposes of the Turkish government, by supplying them with the means of giving an appearance of failure to my experiments, or with a pretext for refusing their support, upon any better plan, to the continuance of my investigation, I could have no apprehension that the character of my proceedings would ultimately be allowed to depend upon a species of evidence, which, if it had even been *bonâ fide*, would, in matters of science, be wholly inappropriate.

One part of the system seemed to be to operate upon my health by every species of mortification. In this the interpreter was necessarily the principal agent. Latterly, he took every occasion to hold me such discourse, as he appeared to suppose would produce this effect. He should not, he said, have been mortified at being obliged to obey a man of superior genius, like one of the French generals; but he felt degraded, that he, a philosopher, and a Candiote, should be commanded by a person of such slender pretensions as myself. Indeed, he did not consider himself as *my* interpreter, but the dragoman of the *Greek nation*. And, pray, said he, one day, “ if we should choose to maltreat you in the

hospital, what could the power of Sir Robert Liston, or Mr. Pisani, avail you?"

These were notices not to be misunderstood, or wholly despised: and I did not neglect, without however allowing the observations to produce the intended effect, to avail myself of the information, which they conveyed. But, as I was not within reach of the means of immediate redress, I thought it most expedient, without entirely slighting, not to resent these insults in any serious manner. I had now, more than ever, occasion to regret my imprudence in not taking arms with me into the hospital: for the sight of them would have kept these cowards in awe, without the necessity of using them; and, if not, their use would have been justified. If any person should again have a desire to place himself in a similar situation (which, for purposes of investigation, has, I trust, by these experiments, been rendered unnecessary) I would counsel him to go armed to the chin, as I understand was the case with my predecessor, Dr. Valli, who besides was accompanied by an entire retinue of confidential persons; notwithstanding all which precautions, and the powerful protection of Prince Murusi, he appears to have been, like myself, very ill treated, especially in the article of subsistence.

After these scenes, I seldom admitted the interpreter, even in the day time, into my chamber,

and had recourse as rarely as possible to his official assistance. The necessity of being always on the alert, in order to defeat the machinations of these miscreants, did, I think, contribute to preserve, or to increase, the vigor, both of my body and mind ; which was farther aided, by the constant exercise, which I took, in the open fields ; for, I never remained in the hospital during the day, but whilst I was visiting the patients, or writing my journal, or when it rained, which was not often. Besides the advantages, to my health, of being perpetually in good air, I judged it might be, in other respects, conducive to my safety, to be often seen in the fields, and in the adjacent burying ground, through which some of the inhabitants of Constantinople were frequently passing, and which they used as a resting place, in the heat of the day, or a lounging place, in the cool of the morning and evening.

This would render whatever plans might be formed by the conspirators, both more difficult, and more dangerous in the execution. They did not again assemble near my door. They probably now, for a time, abandoned their design, trusting perhaps that the starvation, and mortification, which they were enabled to inflict, would occasion a relapse of the malady, which would bring me more completely within their power. And this would undoubtedly have happened, had

I remained, as they were endeavouring to contrive that I should do, a month longer in the hospital.

Earlier on the same evening, the Papa Niofité, being, perhaps purposely, in a state of ebriation, had attempted, under colour of a fraternal hug, in a pretended fit of enthusiasm, to introduce something, which was, or he no doubt imagined to be, deleterious, into my ear, exclaiming, in bad French, "What a pity it is that two great men should not be able to understand each other?" Finding the finger of his right hand busily employed, as if seeking the entrance of my left ear, I thrust him from me with some violence; and, not being very steady, he nearly tumbled head foremost down stairs. It was doubtless in pursuance of the same design, that the subsequent meeting, and deliberations, were held in the evening.

Although it seldom, I believe, happens, that Mahommedans are to be met with, in a menial capacity, with Christians, there was, at my entrance, as the interpreter informed me, a Turk, amongst the servants of the Pest House. He was, by far, the most decent person in the establishment; worked more than all the rest put together; was obliging, steady, and sober; and understood attendance upon the sick. I believe he also acted in the capacity of cook.

I had occasion repeatedly to remark this man's respectful and considerate conduct. Having promptly cured him of a severe cholera morbus, he ever afterwards manifested the most marked gratitude. I could not prevent him from literally kissing my shoes. Having, for some days, missed this worthy Turk, I made particular enquiries after him, but could not ascertain whether he was dismissed, or went away, or what had become of him. The interpreter either did not, or would not, understand the questions, which I asked, respecting the manner of his disappearance; and, in reply to them all, I could only get a shrug of the shoulders, and a "Dieu fait tout." In this matter, there was a mystery, which was not calculated to lessen my unfavourable surmises, respecting the intentions of the other inmates of the hospital.

I had taken the precaution to provide ten, or a dozen, calico under shirts, which I found of extraordinary utility, during my illness. To shift frequently, under the copious perspirations which I experienced, was not only comfortable, but essentially beneficial. I changed these under shirts every two or three hours, or oftener; and when I could not get them washed with sufficient quickness, I dried those I had taken off in the sun, and wore them again in succession. I think it essential to those, who may be similarly situated, in respect to this, or any other acute malady, in which

copious perspiration may be a symptom, especially in a warm climate, to be informed of the great benefit, which I derived from this practice.

There is little variation either in my symptoms, or their treatment. I continue progressively, but slowly, to improve. This day, Hogia Meker ditch, an Armenian of good understanding, who had given me some lessons in the Turkish language, previous to my entering the hospital, visited me, along with an Armenian merchant, who came for medical advice. We held our consultation in the burying ground; neither of them seeming to be much afraid of approaching me. The former, who appeared to feel a warm interest in my fate, cautioned me strongly against remaining any longer in the hospital. I thanked him; and, his notions upon the subject, coinciding perfectly with my own, I requested him to deliver a strong message to Mr. Pisani, and to make my wishes, and my situation, known to several gentlemen at Pera, as well as at the British Palace, which he promised faithfully to perform, and I have reason to believe punctually executed.

Dr. Macguffog, whom, according to Mr. Pisani's letter of yesterday, I expected to-day, has not yet arrived.

CHAP. XXVII.

New mode of counteraction—resolved to take charge of no more patients—visited by Dr. Macguffog—press Mr. Pisani again on the subject of my removal—mode of carrying suspected letters at the end of a cleft stick—the Apothecaries of Pera abandon their design of waiting upon me by deputation—Death of Captain Spiraki—attempt to appropriate his watch—death of a young female patient—Greek burying ground, and adjacent walks—interests, conduct, and policy of the Directors—they remonstrate against my leaving the hospital—intrigues of the Levant medical practitioners—strangulation of Dr. Lorenzo—gates of the hospital kept shut against the Turks—letter from Mr. Pisani,

TUESDAY, August 29. Found Nichola, of Constantinople, who was admitted yesterday, in articulo mortis, having been all night, purposely I must presume, exposed to a current of air, from a sharp north-east wind, with rain at times, pouring upon him through shattered windows. This was a new, but a very efficacious mode of counteraction; and, there were no doubt farther expedients in reserve. I therefore resolved, from this day, to take no more patients under my care in this hospital; continuing, however, to attend those who were already on my hands. Wrote a letter to Mr. Pisani to this effect, and repeating more pressingly my wish to be removed. But

Piero, his servant, from fear, either real or feigned, refused to take the letter, observing that another servant, who should come to-morrow, would take charge of it. In the mean time, Dr. Macguffog, whom I had expected yesterday, made his appearance; and as he could not take my letter for fear of being put in quarantine, as a suspected person, I read to him the contents, requesting that he would communicate them to Mr. Pisani. This letter was conveyed, on the following day, at the end of a cleft stick, which another messenger of the dragoman reached to me across a ditch, and afterwards held up in the air, marching in this way through the streets of Constantinople, to the great amusement, I should think, of the Mahommedan population. Thus two days more were destined to pass, before I was to be relieved from my state of purgatory. Dr. Macguffog informed me, that the apothecaries of Pera, upon hearing of my illness, had relinquished the design of waiting upon me by deputation.

This day died Captain Spiraki, of Cephalonia. I afterwards learnt that the relations of this unfortunate man had some difficulty in recovering his watch from the servants of the hospital, notwithstanding the interference of the British legation, whose protection they claimed.

Wednesday, August 30.—The young girl died yesterday. Missail of Dunapé, and Andria of

Cyprus, who are both doing well, are now the only patients of the old list remaining in the hospital.

Passed not so much a restless, as what might be called a visionary night. The use of the warm pediluvium with nitric acid, was not succeeded by that benignant sort of perspiration, and comfortable feeling, which I have generally observed to follow its application. I got up early, and arranged my papers, in order to give them a chance, in case of accident, (not that I had any apprehension of an unfavourable issue from the malady) of being rendered of some use to the world; although, indeed, I could scarcely indulge the hope, that, in such case, they would ever be permitted to see the light.

Having finished this task, I took some turns in the Greek burying-ground, my favourite promenade, having the recommendation of being pleasantly situated in the immediate vicinity of the hospital, to the west, dry, and well shaded, during the heat of the day, by the umbrageous foliage of the walnut-tree, the neighbouring eminences commanding extensive and beautiful views of the sea of Marmora, its islands and shipping, of the city of Constantinople, and of the picturesque suburb of Scutari. On one of these eminences, distinguished above the rest, distant a few hundred paces due west from the burying-ground, I used to sit down, after being fatigued, to ruminate on the various incidents which were passing, amused

alternately by the rich *coup d'œil* around me, and by reflections on the peculiarity of my situation. They naturally suggested to my mind the similitude, in one respect, between it, and that of the prince, mentioned in *Candide*, for whom Miss Cunigonde was washing dishes on the banks of the Propontus, and who had "but few dishes to wash."

In the afternoon, had a visit from Mr. Stavrach, the treasurer, and several others of the directors of the Pest Hospital. These persons were wont, from time to time, to honour me with a conference outside of the walls. Our ordinary place of rendezvous was the adjacent burying-ground. They used to take their stations to windward, and at regular distances, which were greatly extended upon my illness being declared. The interpreter occupied an intermediate position. They assumed the air of patrons; professed themselves extremely well satisfied with my conduct, and hoped I was equally well satisfied with theirs. To this I evaded giving a reply, convinced, by this time, that, although I might have abundance of professions, neither remonstrance nor complaint could produce any melioration either of my own treatment, or of the prospects of the patients.

It might perhaps be supposed, as these directors never enter the Pest Hospitals, that they can know nothing of what is transacting in the interior, excepting through the medium of inferior

agents, interested in deceiving them ; and that they might have concluded that I had been experiencing, from these inferior agents, the most assiduous attentions, the most civil treatment, and the best possible fare, whilst I was uniformly neglected, often insulted, and, during the few days that I could eat, starved. It might indeed have happened that, if the Turkish government, and the directors of the Pest Hospital, had both sincerely favoured my investigation, the inferior agents should, notwithstanding, prompted by their own particular views of interest, venture to employ means for frustrating my success. But, as the Turkish government, however they might have thought it necessary politically to appear to favour my researches, were known to be decidedly hostile to their success, the directors receiving their orders from the dragoman of the Porte, would not, even if they had no personal motives for counteraction, dare to act but in the sense of their superiors. As a body, however, the directors had themselves powerful motives for wishing my investigation to fail : and to expect that any body of men will act with integrity, and public spirit, when the interests of their daily vocations are placed in direct opposition to those of humanity, is to go counter to the uniform experience of the world. Few will be so foolish as to doubt, that the discreet directors of the Pest

Hospital, like Signior Manuel Ordonnez, actually got rich by managing the affairs of the poor; and it is notorious, that they are persuaded they can only preserve themselves from pestilence, by the removal to the depôts, which is tantamount to the destruction, of their pestiferous dependants.

Hence, besides the horror, which they might feel at my avowed disbelief of contagion, their particular motives for opposing, must have greatly preponderated over those for supporting my views. And, if to this be added their knowledge of what was agreeable to the Porte, and accorded with the personal interests of the Grand Seignior, whose taylor was a principal member of their body, it would be extreme folly to suppose, that, in the perverseness of their conduct to myself, the executive agents of the hospital were not acting implicitly according to the understood wishes of their superiors, as prompted both by their sentiments, and their orders; or that they were not all acting in unison, in order to frustrate my object.

If this had not been the case, or if the directors had been only indifferent as to the fate of the enquiry, is it, for a moment, to be imagined, that they would not, for the sake of common decency, and common fame, have, instead of empty professions, used some exertions to have provided me with a proper set of attendants, good accommo-

dations, and wholesome and suitable food? Instead of a fanatical and presumptuous coxcomb, not understanding a dozen phrases of either English or French, they would have taken care, on such an occasion, to have procured an interpreter, who knew something of the languages which were necessary to be spoken¹. They would have

¹ It was the custom of this person, in order to frighten away fear, especially after he saw me attacked with the malady, to read his Greek testament aloud, and to drink Rackey. He had the name of God eternally in his mouth. "Dieu fait tout; Dieu fait tout;" was the sum of all his philosophy. He piqued himself upon being a Candiot, or native of Candia. This personage will, I doubt not, upon the strength of the present ordeal, be at some future period found figuring among the medical tribe of the Levant. Such is the unhappy fate of the inhabitants of that fine country, that they are, for the most part, doomed to suffer under the lancets and the poison of fellows, who have, by mere accident, been, for a moment, menials in the service of men of science; or of Greeks, who have, at a trifling expence, acquired a few medical terms in the schools of Italy. There are, indeed, some Italian, and a few French and German physicians and surgeons, of a better description; and Dr. Macguffog, a regularly educated physician from Scotland, has recently settled in Constantinople. But, in that great city, which approaches most nearly to London, and to Paris, there is not an individual of regular medical education, either physician or surgeon, who will approach a patient ill of the plague, at once the most frequent and the most fatal malady incidental to that part of the world. Hence the care of pestiferous patients devolves entirely to the priests

been anxious to appoint a distinct culinary and personal attendance, instead of leaving me de-

of the different nations in the Levant, who have in general strong nerves, and are not frightened for contagion, or Turkish surgeons, who do not believe in it; and they, besides administering spiritual consolation, only attempt to treat buboes and carbuncles, without venturing or knowing how to attack the main body of the malady.

In general, the professors of the medical art, at Constantinople, gain more money by intriguing, than by the exercise of their adopted calling. In the course of their professional visits, they are frequently bribed to convey notes, respecting political intrigues, privately, from one Turk to another; and, when detected, they, or their employers, or both, are strangled, or beheaded, if they should not have time to escape. This fate happened, not long ago, to an old Italian physician, who, although known to be a great intriguer in this way, was otherwise held in some degree of esteem. After having had several warnings from the Porte, thinking himself secure in being a favourite, he still persevered; but, at length, being sent for, one day, by the Grand Vizier, he did not return. His body was found, next morning, in the great burying-ground of Pera, under the British Consul General, Mr. Morier's house, with evident marks of strangulation. He was discovered to have been accessory in carrying on, by means of these notes, intrigues in favour of the Prince of Wallachia against the Porte. Wishing to make the murder pass for an act of robbery, and private assassination, the Turkish government have caused a building to be erected, upon the spot, on which the body was found, in order to serve as a guard-house, and to be at once the means of preventing future murders, and of commemorating the melancholy event. The stratagem imposes upon no one. The name of

pendent for these services, upon the ordinary domestics of the hospital, whom it could not be very pleasant to see, after dragging pestilential patients in their arms, coming to wait at table.

It was on this day that the directors presumed to express their surprise at learning my intention to quit the hospital. They earnestly requested me to remain another month. The extraordinary proposition, that a person, not yet recovered from the plague, should continue to attend patients, labouring under it, for another month, was calculated to excite strange surmises in my mind; and I gave them an unqualified refusal: for, the anxiety manifested by the directors, by the interpreter, and even by the inferior agents, for

this unfortunate physician was Lorenzo; and people in general regretted, although they admitted, that he, in some measure, merited his fate.

Another instance of a similar detection still more recently occurred. A Turkish nobleman had a note conveyed to one of the favourite sultanas, promising a certain considerable sum, if, by her influence, the Grand Seignior would confer upon him a particular appointment. The note was deposited in the bottom of a pill-box; but, before it was opened by the lady, a mischievous eunuch had the curiosity to pry into the box, and carried the note to the Grand Seignior. The nobleman was beheaded, and the practitioner of medicine took to flight. It is usual for the persons, who act in this double capacity, when they become strongly suspected, to sell off their property secretly, and to quit the country.

my acquiescence, amounting even to remonstrance, left me no room to doubt, that some favourite object remained to be accomplished, which could only chance to be effected by delay.

I observed that the servants of the hospital were always very anxious that the gates should be kept constantly shut. It frequently happened to me, upon going out to take a short walk, to leave the side gate from the yard open, in order to have free ingress, and egress, without having, upon my return, to wait for admission from within. I invariably remarked that this was instantly noticed, and the gate immediately shut; but, that, upon knocking for re-admission, it was never opened but after much delay. Respecting the grounds assigned for this precaution, alledged to be directed against the Turks, I was not satisfied. They could not surely be apprehensive, that the Turks, from mere wantonness, would offer them any personal violence. They could not be alarmed, lest they should plunder them; for they had nothing to lose, but a wooden spoon, and an earthen pitcher, for each patient. But they might very well dread, that the Turks, or other passengers, who, seeing the gates open, might be curious to look in, would note and represent the wretched state of their sick; the little care and attention that was paid to them; and the miserable rice water, which constituted

their sole subsistence. Such, I am persuaded, were the true motives of this unusual vigilance.

In the course of the day, I received the following letter from Mr. Pisani :—

Pera, Wednesday, 30th August, 1815.

My dear Sir,

I was very glad to receive a good account of your state of health by the gentleman who conversed with you yesterday, and who made me acquainted with the nature of your wishes. I set about it without a moment's loss, and it is only at present I can acquaint you, that, after much difficulty, I have found a small house belonging to an Armenian, at some distance to the north of the British palace, which the owners are willing to let you. It has no furniture, which should make no obstacle ; for you can get a table and some chairs from your friends for the time of your quarantine. But I am only vexed that I cannot know till to-morrow, what rent they mean to ask ; for the husband of the lady, upon whom that point depends, was absent, and does not return till late at night. At all events, I do not think the demand can be very extravagant, considering the distance and the smallness of the house.

Consider of it in the mean time, and I will endeavour to arrange the rest as conveniently and satisfactorily to you as I possibly can.

I remain very truly,

My dear Sir,

Your faithful humble Servant,

B. PISANI.

P S. You have many compliments from his Excellency Mr. Liston, and every one else at the palace.

I know not whether Dr. Macguffog, or the language-master, be the person alluded to in the first paragraph of this letter; for I sent messages by both. The matter in question (the rent of the house) was only sixty piastres a month; and if it had been ten times as much, it ought not to have been a matter of one moment's consideration, particularly as the expence was to be defrayed from my own funds; and as it was known that I considered my life in danger, although not at all from the disease, every moment that I remained in the hospital. A relapse and delirium might have occurred; and then certain death would have ensued. I shall afterwards shew, as far as our experience has extended, that, to be seized with

the plague, is an invariable consequence of residing in a pest-house, in the Levant, as an investigator; and, that, although those, who are not affected with delirium, may escape, those, who are, must certainly die, independently of the disease. It was now nine days since I first expressed my wish to be immediately removed from the hospital; and, in order to account satisfactorily for the delay, I must suppose, that, besides the difficulty of procuring quarantine quarters, Mr. Pisani must have been unaware of the nature and extent of the various dangers to which I was, by such delay, momentarily exposed.

It will not be suspected, that I can delight to dwell upon these circumstances, with any retrospective view. But, when persons of the highest general qualifications, and of the first local knowledge, are found so little conversant with the nature of these institutions, it becomes a more imperative duty upon those, who have had personal experience of their atrocity, to make such an exposure of their principles and conduct, as must lead to their entire abolition, and consequently to the greater safety of the poor pestiferous patient, as well as of the zealous medical enquirer.

CHAP. XXVIII.

Quit the hospital—sorrow and mortification of the executive agents—arrive at quarantine quarters at Pera—my immediate wants supplied by a kind-hearted Janissary—hospitality and liberality of the Turks individually—supper brought from Josepini's hotel—pass the night alone—sleep sound—visited by my language-master—walk out with him—not afraid to approach me—visited by Mr. Pisani, Mr. Black, and Dr. Macguffog—reports circulated of my death—assigned period of quarantine—ludicrous trait of a sailor hired as a servant.

THURSDAY, August 31.—Took my draughts as usual in the night, and slept quiet, though not sound, and without wandering. Awoke with a good appetite, but could not, for a long time, get any thing to eat. At last, between eleven and twelve o'clock, succeeded in procuring some rice and milk, of which I made a hearty, although a simple repast. It may be remarked, that, although the servants of the hospital well knew, that this was one of the very few dishes belonging to their table, of which I could partake, and although it was abundant, cheap, and a regular supply easily to be obtained, they never served

it, after they perceived that it was not disagreeable to me. Here there was at least the semblance of a regular system.

At length, between five and six o'clock, P. M. I received the following letter from Mr. Pisani :

Pera, 31st August, 1815.—Thursday, 1 p. m.

My dear Sir,

I have received your letter, and I am happy to hear you are well.

I have taken every step in my power to get a house, which must do for the moment as it is ; for it is incredible the objection of people in general to let their houses, where the name of the plague is mentioned.

I have fitted up one room for you, with a bedstead, bedding, &c. and I send you a Janissary to conduct you thither. You must take as few things as you can in the first moment, for the purpose of keeping your new habitation as clean as practicable, and any linen, &c. we may get brought, and washed afterwards.

Farewell, my dear Sir. Keep up your spirits, and believe me ever,

Your obedient and faithful servant,

B. PISANI.

P. S. The Janissary carries two boats for your service, and I have ordered a chicken and a fowl for you this evening.

At the receipt of this agreeable intelligence, it did not require many minutes to prepare my baggage. It was with real sorrow that the inmates of the hospital saw me depart definitively from within their walls, not only living, but almost in perfect health. They, however, strove to dissemble their mortification. They all became in a moment extremely respectful. The interpreter even affected some regard. On the road to the water side, to which I walked with a firm step, chagrin and resentment vanished, and hilarity was restored to my breast. A few remaining rubies (a small gold coin) were transferred from my pockets to those of the train, who accompanied me, carrying the baggage to the boat; and I consoled the interpreter by bequeathing to him as a legacy some excellent wine, and English bottled porter, which remained unconsumed, and to which I knew that he had no dislike. A fourth time I passed through the Butchery, and embarked under the wall near the Seven Towers.

Landing at Topanha, between seven and eight o'clock in the evening, I walked, at a good pace, behind the Janissary, who conducted me to a lone house, situated at the farther end of the street of Pera, nearly a mile and a half from the water side, and destined for my quarantine quarters. Having opened the door, and ushered me in, he shut it, and disappeared, leaving me to my meditations.

This was a new situation. I was quite alone in the house. Neither neighbour, servant, or interpreter. It was dusk. I might go out to be sure ; but I could not make myself understood, nor supply my wants, although hunger was pressing. From a small public-house adjoining, I tried, by means of signs, and offering money, to procure coffee, or some eatables ; but, the inhabitants being Greeks, and having learnt who I was, the women and children ran away screaming at my approach, and I could get no one to attend to me. From this state of perplexity, I was not relieved, until the Turkish police came to make their rounds of the district between eight and nine o'clock. Two of them came up to my quarters¹. One, whom I understood to be the superior officer, after giving orders to the other, which I supposed to consist in seeing that I was properly guarded, retired. The Turk, who remained, shewed me the greatest civility, went of his own accord for coffee and a pipe, and seemed solicitous to display a marked zeal to render me service. He staid for half an hour. His curiosity led him to examine minutely every article in my apartment, which was strange to him ; and

¹ The Turkish police has very little employment. In Constantinople there are few thefts, robberies, or murders, in comparison with any other large city, in which I have ever resided.

he tried on one of my boots, but without manifesting any propensity to appropriation. This kind hearted Janissary (for such he was) called upon me repeatedly afterwards in his rounds; and, although we could not understand a word of what each other spoke, I could perceive, that he felt an interest, arising probably from the nature of the service in which I had been engaged, that I should be made comfortable in my comfortless habitation. From several traits, which I have had an opportunity of observing, I am persuaded that, individually, there is much hospitality, as well as liberality, to be met with amongst the Turks. Between nine and ten o'clock, an Armenian porter brought a boiled chicken, which had been prepared for me at Josepini's hotel, and, having deposited it on the threshold, withdrew. This was my first comfortable meal for seventeen days; and I eat, or rather devoured, the whole chicken. The porter brought a message that no person could be got to-night to attend upon me; but that Mr. Pisani would endeavour to procure a servant in the morning. Having finished my supper, I prepared to go to bed. Upon examining the fastenings, I found them such as would not be deemed very safe in London, or Paris, although they might be sufficient at Constantinople. My mind was considerably exhilarated by the reflection, that, since I had escaped,

as if by a species of miracle, from the sepulchre near the Seven Towers, the results of my researches might possibly be rendered of some service to mankind; and I retired to rest, with the comfortable assurance, that, although alone, in a strange house, and in a strange city, I should be able to sleep sound.

Friday, Sept. 1st. Took my draughts as usual, and passed a good night. Arose early in the morning, and arranged my clothes, and papers. Was visited by Mr. Meckerditch, my language master, who appeared much rejoiced to see me safe out of the Pest Hospital, and did not seem to have the smallest dread of approaching me¹. We walked out, and had some coffee together. But he requested me not to mention the circumstance; as, if it were known, he would be shunned as *an infected, or at least a suspected person*. Soon after my return, Piero, Mr. Pisani's servant, arrived, and informed me that his master, Mr. Black, and Dr. Macguffog, would shortly call at my lodgings, and requested me not to go from home. Mr. Pisani arrived at twelve o'clock, and promised that a person should be procured to attend me, in the course of the day. In the mean time, it was arranged that my dinner should be sent daily from Josepini's hotel; and that it should con-

¹ Of all Christians, the Armenians, I believe, are the least afraid of the plague.

sist, till farther orders, of a boiled chicken, with potatoes, and other vegetables. I had some of my stock of liquors still remaining. My friend, the police Janissary, again called, and renewed his civilities. No servant being yet got, the same Armenian porter, who brought the supper last night, brought the dinner to-day; and, depositing it on the threshold, as before, withdrew. In the afternoon, I was favoured with a visit from Mr. Black, and Dr. Macguffog. The latter informed me, that, on his return from seeing me, on Tuesday, at the Seven Towers, he was assured, by several persons, at Pera, that I had died on the preceding Monday, at two o'clock; and that he durst not contradict the report, on the ground of his having just seen and conversed with me, lest he should be himself shunned by his patients, as a suspected person. According to some, I had died of the plague, and to others, of poison, administered by the Papa. How far these reports might have originated from, or been connected with, the suspicious circumstances of Sunday night, or with my previous denunciation, and the consequent removal of Parthenius, it is now, and will perhaps remain for ever impossible to determine. But, that they were not mere accidental rumours, but reports at first sent abroad systematically, and with design, and afterwards propagated, in good faith, by the credulous and

unwary, are rendered, by subsequent occurrences, as well as by the known tactics of the dealers in plague, more than probable.

Saturday, Sept. 2nd. Viewing the separate portions of Constantinople, and of its suburbs, from the bottom of the hills, upon which they stand, it may be compared to a town, which had been bombarded by an enemy, or a fleet, which had been dismantled by a storm. The houses are generally large, and built of wood, with numerous windows; many of them being in a condition, which we should, in London, consider uninhabitable. The miserable tenement, which was procured for me as quarantine quarters, is situated over a brook, on the declivity of the valley, which divides the suburb of Pera from that of Demetri, and extends from the Artillery Barracks to the water side, at Galata. It consists of four rooms and a kitchen, each of the larger rooms having seven windows, and none fewer than four or five. The windows have frames, but scarcely any of them, with the exception, partially, of that in which I sleep, has got glass. Some of the squares are filled with pieces of deal, and some with brown paper; but by far the greatest number are entirely open. The roof is not, in any of the apartments, water-tight. The house is overlooked by the passengers on the public road, from Pera, to all parts

of the country. They frequently stop to satisfy their curiosity. It is exposed to all the winds of heaven, without the shelter of a tree, or, excepting at one corner, of any neighbouring dwelling. Certainly, for purposes of purification, which I must presume my friend Mr. Pisani to have principally had in view, nothing could have been more exquisitely selected.

It seems it is much easier to get into a pest house, than to get out of it. I could not, speculatively, have had any idea, that, in a Turkish community, there could have been so much difficulty in procuring a house, or lodgings, for the purposes of quarantine, or even for the accommodation of persons actually suffering under disease.

Had I been a Turk, I should not have been subjected to the inconvenience of quarantine. Nor was it compulsory on me, in Turkey, being a Christian. But, as it was according to the opinions and the wishes of the British Ambassador, and the Factory, I determined, however absurd such an ordeal appeared to myself, to submit to the longest period of quarantine, which could reasonably be exacted of me. Forty days, *from the commencement of the disease*, was the only period that could be indicated, as founded, according to the doctrine of contagion, upon any given principle; and to this I assented.

This matter being arranged, another difficulty was experienced. A proper servant was not easily to be procured. Last night one of those vagrants, half Italian, half Greek, who are to be met with in the streets, nearly in a state of nudity and starvation, was sent to me. He had been a sailor by profession, and must have been induced to undertake this new employment, I should presume, by hard necessity; for, his very first act, in my service, was to snatch a bottle of porter out of my hand, of which I had just drawn the cork, and, raising it with the rapidity of lightning to his head, to begin to drink. There was something at once so *naïve* and so ludicrous in this exploit, as to throw a shade over the indecency of it, and to move me from my gravity. As his vicious propensities appeared to be of a nature dangerous only to himself, I determined, as preferable to being alone, to tolerate him until a less unfit person could be procured. By a species of *lingua França*, with the frequent intervention of signs, we made shift to understand each other; and, although in a constant state of intoxication, he contrived to execute the commissions with which I thought fit to entrust him, with as much fidelity and dispatch, as could reasonably have been expected.

CHAP. XXIX.

Hire a new servant—receive visits from Messrs. Morier, Black, Barbaux, and Dr. Macguffog—we converse at a distance—Sir Robert, and Lady Liston, honour me with a conference.—It continues to be asserted that I am dead, and that the Ambassador, and the English, are concealing the circumstance.—Mr. Smith, and other gentlemen, favour me with calls—they all proffer their assistance.—Mr. Pisani brings Don Courban, who gives his advice—estimation of his experience—visit from the interpreter—liability to relapse—my cellar and pantry well stocked—my health returning to its usual state.

TO-DAY, a decent old man, by profession a valet, and who, having had the plague, entertains no apprehension of the malady, offered his services. I hired him, and dismissed my sailor, who went away, with his bedding on his shoulders, with great composure.

Last night, the light being extinguished, I could not see to measure the proper quantity of my medicine. But I slept well, and passed a good night without it, although my sensations are not entirely returned to the healthy standard. I still experience a trifling giddiness, occasionally; but no pain.

This evening, was favoured with a visit from Mr. Morier, the Consul General, who, very kindly enquired into the nature of my wants, proffering me, most cordially, money, clothes, (supposing my own to be all *infected*,) wine, chocolate, and whatever else he conceived, might contribute to the restoration of my strength, and spirits. This benevolent gentleman, who, I regret to add, has recently paid the great debt of nature, appeared truly glad to see me once more alive.

I was also favoured with a visit from Messrs. Black, and Barbaux, and Dr. Macguffog, who were all anxious to alleviate the *ennui* of my solitude. They conversed with me from an eminence, at some distance, whilst I stood at one of the upper windows.

The Mohammedans are the only inhabitants of the Levant, among whom the terror inspired by the prevailing errors, respecting the cause of the plague, do not suspend all the affections of consanguinity, and sever every social tie. They alone do not desert their friends, or relations, who have the misfortune to be seized with this dangerous malady, or consign them to those sepulchral tenements, misnamed pest *hospitals*. On the contrary, they fearlessly administer to them, at home, whilst life lasts, all the relief, which it is in their power or knowledge to bestow. If this conduct of the Turks were even dictated

solely by religious prejudice, whilst that of all their neighbours is the effect of temporal terror, it could not be denied to be far more respectable in its motive, as it is undoubtedly more beneficial in its consequences to society. The question, in a view of science, is decided in favour of the Mahommedans: and, I expect, that, shortly, the Christians will be ashamed not to follow, in this respect, their excellent example. Then, will the simple, but affecting, description of the poet, happily cease to be applicable:

————— The sullen door,
Yet uninfected, on its cautious hinge
Fearing to turn, *abhors society*,
Dependants, friends, relations, Love himself,
Savaged by woe, forget the tender tie,
The sweet engagement of the feeling heart.

Sunday, Sept. 3rd. I was honoured, in the afternoon, with a visit from Sir Robert and Lady Liston. Sir Robert informed me that it still continued to be asserted in town, that I had died on Monday; and that he and the rest of the English were concealing my death. That such an event had been reckoned upon, if we advert duly to the circumstances that have been stated, there cannot, I think, be the smallest doubt. Mr. Black, and the gentlemen in his counting house, and Mr. Smith, a member of the factory, called in the even-

ing. Mr. Morier, with his usual kindness, sent me a supply of chocolate. My new servant, Francisco, understands how to make coffee, chocolate, and lemonade, in a superior manner, which I find very useful, as I now take these dietetic articles fr  ely.

Monday, Sept. 4th. Early this morning I was favoured with a visit from Mr. Pisani, accompanied by Don Courban, the superintending priest of the French pest hospital, who was announced to me as being out of quarantine, not having recently had any plague patients under his care, and as possessing an *experience* of thirty years of the malady; i. e. after having contemplated, for thirty years, the variegated symptoms, the numerous deaths, and the few recoveries, which characterize the plague, and attained about as much knowledge of the cause and cure of the disease, as a horse, in turning a mill, may be supposed to acquire of the principles of mechanics, he is reputed a person of consummate skill. I was not to be thus imposed upon, by the misapplication of words. I nevertheless felt much obliged to the dragoman of the British Legation, for bringing a person of such exalted reputation, to examine, and prescribe for me. He proceeded, in the presence of Mr. Pisani, with great solemnity, but at a proper distance, and with a due number of shrugs and grimaces, to inspect my glandular swellings, and my tongue.

He very significantly shook his head : but as I knew that a continuance of quarantine could be the only consequence of the oracular sentences, that might issue from it, I was not very solicitous to enquire into the nature of the augury. He gave me a great many advices, not one of which I followed. He particularly insisted upon my having a seton passed through one of the glands, which I peremptorily refused. He entered into a minute detail of what ought to be eaten, as caviere, olives, &c., and what avoided, as fruit, meat, &c. But, notwithstanding the confidence that the presumed experience of thirty years might be supposed capable of inspiring, I determined, excepting in those matters of etiquette, in which I held myself responsible to the Ambassador, and the Factory, to proceed in the course, which I had originally prescribed for myself.

Tuesday, Sept. 5. Yesterday Mr. Black sent me a fresh supply of brandy ; but I have now lost almost all relish for that liquor, as well as for bottled porter. Have also discontinued chocolate, and rice and milk. At present cold tea and lemonade are my principal drinks ; and small doses of tincture of litta, in water, with the nitric pediluvium occasionally, my principal remedies. Mr. Morier called, and afterwards sent me a

supply of Dardanelles wine. Made an attempt to smoke, but found it disagreeable.

Constantine, my interpreter, or, as he styled himself, "the interpreter of the Greek nation," had the impudence to call upon me. He affected to have been obliged to sleep in the fields, since quitting the hospital, a few days after I left it. As he had served the directors faithfully, this tale was by no means credible. I considered him, at the best, as a spy; and, his intentions being, at any rate, questionable, I would not offer him an asylum, although there was plenty of spare room. After taking some refreshment, he departed; and I desired Francisco never to admit him again. It will hereafter be considered, as one of the incongruities, in the history of the doctrine of contagion, that, whilst I was performing strict quarantine, as a person that was deemed capable of infecting a whole community, my interpreter should have been allowed to go at large. He was to be met with every where in the public walks, and mixing intimately with the people. In this, there was certainly no harm; and I only notice the circumstance, to mark the glaring inconsistency of the system.

Wednesday, Sept. 6th. From the nature of its cause, no affection can be more liable to relapses than the plague; and, in this respect,

no disease can be more dissimilar from those which are produced by a specific contagion. Feeling some degree of indisposition last night, I took, at six, eight, and ten o'clock, small doses of calomel and opium; and, in the course of the night, two small doses of tincture of litta. I am, to-day, better. My friends continue to supply me with wines and provisions, so that my cellar and pantry, or the substitutes for them, are both well stocked. Several of them called in the evening.

It may be proper here to state, that, as it is, at all times, difficult for a stranger, and altogether impossible for a person in my situation, to procure, by purchase, wines, groceries, &c. of a good quality; in order to have them assuredly good, or indeed to have them at all, I was obliged to submit to be indebted to the cellars, and the store-rooms of my friends. But surely never were contributions more voluntary, or more chearful.

Thursday, Sept. 7th. Passed a comfortable night, and slept sound without any medicines. Mr. Morier's excellent old sherry, taken medically, at regular intervals, and in moderate doses, I now find a sufficient and appropriate exciting power. My sensations have very nearly returned to their usual state. Observe that for the last four or five days, my water, which was before scanty, and high coloured, has become quite limpid, and flown in an unusually copious and free stream.

In the treatment of plague, in as far as any is practiced in the Levant, there is a strong prejudice against the use of cathartic medicines, founded, I should conceive, on the frequency with which cases have been observed to terminate fatally, in which diarrhœa has spontaneously occurred, or been occasioned by the abuse of drastic remedies. To this prejudice calomel is particularly obnoxious. The interpreter, and servants of the hospital, were exceedingly curious to ascertain what medicines I was administering to the patients. But I purposely threw as much mystery as possible over the matter. None of them were allowed to enter my room, even to make my bed, but when I was present. In going out to walk, I always locked the door, and put the key in my pocket. When I mixed the medicines, no one was suffered to be present : and I always administered them with my own hands. But, notwithstanding all my precautions, I occasionally missed articles, which I had been compounding. The Papa, Parthenius, as we were visiting the patients, the day before his dismissal, seemed to fancy that he had discovered what he supposed to be my secret, and exultingly vociferated, Calomel ! Upon which the interpreter frowned, shook his head, and appeared to reprehend his indiscretion. If his object was to procure an avowal, or an explanation from me, he was disappointed. I took no notice of the circumstance, but left him in the undisturbed enjoyment of his fancied discovery.

CHAP. XXX.

Write to Mrs. Maclean, to inform her of my illness and recovery—hand not quite steady—anniversary of my marriage—ceased to keep a regular Journal—receive a supply of tea, sugar, honey, &c. from Lady Liston—my other friends continue their bountiful contributions, and their personal attentions—Mr. Mair, unable himself to visit me, sends some excellent fresh butter, a very scarce article at Constantinople—decline medical consultation—transmit a Report of my proceedings at the Pest Hospital to Sir Robert Liston, and Mr. Morier—copy of the Report.

SATURDAY, Sept. 9th. Arose in perfect health after sleeping better than usual. Walked early in the morning on the beautiful eminence, above my quarantine quarters, on which is situated the Turkish artillery barracks, or rather, I might say, palace, commanding the most delightful views of the Bosphorus, the Propontus, the city of Constantinople, the suburb of Scutari, on the Asiatic side of the canal, and, in that direction, even as far as Brusa and Mount Olympus. Recollected that it was the third anniversary of my marriage. Returned home to breakfast; and, the departure of the Vienna post being at hand, wrote a long letter to Mrs. Maclean, informing her, for the first time, of my illness, and, at the

same time, of my recovery. As all my friends at Pera had promised me to forbear writing to England, upon the subject of my malady, until my fate should have been decided, I was in hopes that I should be myself her first informant. I found that I did not write quite so steadily as usual. But I made no doubt that the villainous fumigations, and slashings, which the letter must undergo, before its arrival in England, would render this unnoticeable¹.

From this period, considering my health sufficiently restored, I did not think it necessary to keep a regular Journal; but merely noted down, from time to time, those incidents, of which I wished to preserve the recollection. On the 11th, Lady Liston was so good as to send me an abundant supply of tea, sugar, honey, &c. of the very best quality, from the British palace; and, on the 13th, the

¹ The actual state of this letter, which was of course treated not only as most suspected, but as most infected, will afford a curious specimen, when the preposterous precautions now practised against plague shall be no more, of the manner in which such operations have been heretofore conducted. Mrs. Maclean has promised me to preserve it for that purpose. The usual mode of treating ordinary letters from Turkey, is to prick them, in many places, with an instrument like a bodkin. Those to which suspicion is attached are besides fumigated; and those, which are considered infected, are fumigated and slashed in a most extraordinary way.

principal servants of the Ambassador's household came to wait upon me in a body, to know in what manner they could render me any services. With fresh butter, an article very scarce, or rarely to be met with at Constantinople, my table was liberally furnished by Mr. Mair, a member of the British factory. In the mean time, persons, hearing of my proceedings at the Seven Towers, wished to consult me professionally; and some overtures were made to me by the Armenian community, to induce me to remain, as a practitioner, at Constantinople. These propositions I declined, alledging, that, as I was only come to treat the plague, I did not mean to meddle with other maladies. My countrymen continued daily to call upon me, at such periods as their respective avocations would permit. Mr. Pisani frequently rode to my dwelling, or sent, to enquire after my health, and my wants. My principal wish, at present, was to have the period of quarantine, if possible, shortened, in order that I might be enabled to resume my labours, if agreeable to the Porte, on an improved plan, or to return to England with the results which I had already obtained. But finding, from the strength of the prejudices, by which I was surrounded, that to obtain cheerfully, any diminution of the usually assigned period of quarantine, was wholly impracticable, although, by the authority of Don Courban, after

having examined my glandular swellings, and my general state of health, on the 14th I was pronounced pure; I determined to go on quietly to the end of that period, occupying myself, in the mean time, with writing a Report of my proceedings, concentrated and generalised; of which, on the 18th of September, I sent a copy to his Excellency, the Ambassador, for the information of the British government, and, some days afterwards, another to the Consul-General, for the information of the Levant Company.

The following is a copy of this Report, with a few corrections merely verbal, and the omission of such parts only as have appeared, upon farther reflection, to be incorrect, or superfluous :

“ To his Excellency Robert Liston, Ambassador Extraordinary, and Minister Plenipotentiary of his Britannic Majesty, at the Ottoman Porte, &c. &c. &c.

“ Pera, Sept. 18, 1815,

“ Dear Sir,

“ A very few days after having written to your Excellency, what I intended should have been the first of a series of Reports, on the subject of my enquiries, respecting the plague, I was myself seized with that malady. The duty of self-preservation concurring with inability, to render it necessary that I should, for a time, suspend my

proceedings, I thought fit to remove from the Greek hospital at Constantinople.

“ Being now sufficiently recovered to attend to business, I beg to resume the subject. And, as, in a matter of so much public importance, and, at the same time, so very liable to misrepresentation, I consider it incumbent upon me to address your Excellency explicitly, and without reserve, I think it proper, in the first place, to state, that, even had my health permitted me to continue the investigation, it would have been impracticable to have proceeded in it, either with satisfaction, safety, or success, in the situation, in which I happened to be placed.

(Some pages, relating to the conduct of the directors, and servants of the hospital, are here omitted, the same observations having been, in substance, already given in the Journal).

“ The medical facts, however, which, notwithstanding these untoward circumstances, have transpired, on this occasion, are sufficient to confirm me, in the opinion, that my ideas, respecting the treatment of the plague, will be found to be generally correct; an opinion, which is much strengthened by the cure of that disease, effected in my own person, to which alone, in the situation, in which I was placed, the principles of medical science, could, with any degree of accuracy, be applied. In explanation of this remark,

I must trouble your Excellency with a few medical observations. According to the principles of medicine, which I have adopted, it is necessary that the doses of agents, employed as remedies, in diseases of high degree, should be repeated, at periods corresponding with the duration of the action of each dose. If, for instance, the action of a dose, of any medicine, continue for one hour, the doses must be repeated hourly; if, for two hours, they must be repeated every two hours, and so forth; and this, in the night as well as in the day. Accuracy, in this respect, is particularly necessary, in fevers, plague, &c. where no time is to be lost, in employing all our remedial means. This doctrine, while my understanding continued sound, I could easily apply, in my own case; but it is evident that I could not undergo the fatigue of keeping awake, or getting out of bed repeatedly, during the night, to apply it to the patients of the hospital generally: and there was not an individual under the roof with me, whom I could, with safety, entrust with the smallest delegation of that kind, *even during the day*. I once tried the experiment, in respect to a blister. It was sent, through the interpreter, to the Papa, with orders to apply it to the back of a patient's neck; and, when I came to examine it, some hours afterwards, I found it on a bubo in his groin.

“ Had I, indeed, been able to endure the fatigue of night prescription, I could not have got one person, in the house, to accompany me, to interpret, or, in any other way, to assist. It even gave prodigious offence to all the establishment, that I should have kept them out of bed, for one or two nights, until past ten o'clock. Thus, I found myself obliged to limit my prescriptions to certain periods of the day, forfeiting thereby considerably more than one half of the efficacy of my proposed plan of treatment.

“ Before I proceed to give an account of the malady, allow me to return your Excellency my most grateful thanks for your friendly request, that I should take precautions against infection from the touch, and to assure you, that it is with the utmost deference and respect, I shall always attend to any request, or suggestion of your's, whether in your public or individual capacity. But, in this instance, I hope to be able to exculpate myself from apparent inattention, by observing, that, as your Excellency's note, dated the 19th ult. containing that request, only came to hand at the very moment of my being seized with the plague, if contagion could have been the cause, the rubicon was already passed.

“ Permit me, at the same time, frankly to state to you the reasons, which originally induced me to abstain from taking the usual precautions

against infection, leaving your Excellency to judge of their validity, and to approve or censure my conduct accordingly. As the tongue, and the pulse, but more especially the latter, afford the principal, or almost the sole criteria, by which the degree of danger, and the changes operating by the remedies, can be correctly judged of, in acute diseases, it has appeared to me that not to attend to the state of these organs most accurately (an accuracy, in respect to the pulse, incompatible, as I think, with precautions) would be doing matters only by halves. Farther, without regard to the various opinions entertained respecting the cause of the disease, I flattered myself, from a strong confidence in what I had, on other occasions, observed to be the invariable result of the due application of scientific principles, in the treatment of severe maladies, that, in the event of my being attacked with the plague, I should probably experience a speedy recovery. It has also been an opinion of mine, that it is as disgraceful, for a physician, to avoid personal dangers, arising from disease, as for a military officer unnecessarily to turn his back upon an enemy. Nay, I cannot help thinking it would tend much to the improvement of medicine, were the members of the faculty themselves more frequently seized with those diseases, of which the nature, and the cure, continue to be

involved in obscurity : and, upon these grounds, I would fain hope that, neither the example of my personal exposure, nor the attack of the malady, which I have experienced, will be wholly without their use.

* * * * *

“ I now proceed to give some account of the disease, and of the plan of treatment which has appeared to be most successful, as these have come under my own observation, and as they have been recently deposited in my notes, and in my memory.

“ The most prominent phænomena of the plague indicate a considerable affection of the brain and nerves, according to the degree of which principally, I think, the degree of danger, in each case, is to be estimated. The affections of these organs are manifested, in the early, and milder stage of the disease, by a rapid and unconnected succession of ideas, a hurried speech, a tremulous and unsteady walk, and a distracted look. The appearance of the countenance is peculiar. It more nearly resembles the desultory and wandering look, which takes place in the typhus of Batavia, and in the jungle fevers of other parts of the East Indies, than the fatuitous and fixed stare, which I have met with in the yellow fever, and some other febrile affections of the West Indies, and of Europe. The skin and muscles of the face exhibit a tremulous appearance, which, with

a certain fierceness of the eyes, and sometimes an involuntary motion of one of the eye-lids, as in winking, gives a peculiar expression of countenance, the whole constituting rather a ludicrous wildness of aspect, which seems to be increased, by the efforts of the patient, as if conscious of the insubordination of his features, to preserve their composure.

“The alienation of mind, which accompanies, or succeeds, this slighter affection of the nervous system, is sometimes accompanied by a distinct consciousness of the existing perversion of the ideas. In the notes upon my own case, it is thus described: “The state of mind, which I experienced, at the commencement of the disorder, consisted in a species of agreeable alienation, during which all the mutations of the universe seemed to pass through it, in the shortest space of time. This state returned every night, or when I laid down to sleep in the course of the day, for upwards of a week. In the mornings, or upon getting out of bed, or having my attention solicited, my mind returned to its usual state; but, it was unable to recall any of the impressions of the night, although the conviction that numerous impressions had been made, most fully existed.”

“Although the state of mind which I have described, in my own case, does not of itself

indicate any considerable danger, yet, since it is so very apt to rise into delirium ; and since delirium is a symptom always denoting great danger, in this disease, I am of opinion, that, even in the most trifling alienations of mind, the remedies of delirium should not be delayed.

“ The affection of the circulating system was considerable in all the cases of plague, which came under my observation ; and the type of the fever seemed to be, as in my own case, for the most part continued, with exacerbations, although I have observed it remittent, and I have no doubt it is also sometimes intermittent. The pulse was usually very small, and not unfrequently irregular. In ordinary cases, it beat from 108 to 112 times in the minute ; but, in those of imminent danger, its quickness was commonly about 130, and sometimes it was not to be counted. This was the case with Tumus of Roumelia, who, however, recovered. And there is no doubt, that, under scientific treatment, of moderate accuracy, patients will frequently recover, in all kinds of fever, after their pulses have exceeded a hundred and thirty.

“ Upon the whole, the fever, which constitutes so essential a part of this disease, appears to resemble more nearly that, which has been commonly known, in nosological systems, under the

denomination of typhus, typhus gravior I mean, than any other.

“The appearance of the tongue, in plague, is very various. It is sometimes blackish, sometimes brown, sometimes quite white, and now and then of a flesh red, always, in severe cases, dry and hard, and most commonly covered with a thick crust, the colour of the crust generally corresponding with the colour of the organ for the time being. I have also seen it of a glossy grey, resembling the first formation of icicles upon water; and this state, *cæteris paribus*, has appeared to promise the most favourable termination. My own tongue, notwithstanding that all the other symptoms of consequence were almost completely removed before the twelfth day of the disease, continued of a darkish brown colour, dry, and with a disagreeable bitter taste, especially about the root, till the eighteenth day.

“At the commencement, as well as in the progress of this malady, the bowels are generally extremely constipated; and patients have stated, upon coming to the hospital, that they have been four, five, six, and even seven days, without having had a motion. Captain Spiraki, of Cephalonia, was in this latter predicament, although he had, previous to his admission, been attended by

an apothecary at Galata. But, when the disease has long existed, and the patient has become maciated (a consequence which necessarily happens under the usual mode of treatment) a diarrhoea is very apt to occur.

“ Much has been said of the intolerable nature of the fœtor, emanating from pestilential patients. Respecting the peculiarity of the fœtor there cannot be a doubt. But, as to its nature, although it be certainly disagreeable, yet I cannot say that it struck me as being, in any sensible degree, more so, than that which emanates from persons labouring under fevers of great severity, and of long continuance, of any other kind, or in any other climate. In diseases of intensity, the fœtor is always strong, and always peculiar, according to the nature of the organs more especially affected. I even very much doubt, whether, if the same order and cleanliness were observed, in both situations, the smell of a plague hospital, at Constantinople, would be found more disagreeable, than that of a general hospital in London.

“ I have now enumerated the principal affections, which I have myself observed to constitute the disease called plague. Among them, vomiting has not been particularly mentioned, although I believe it is stated as a frequent symptom by authors, because I have not known it to happen in any case, excepting in my own. This took

place on the first day of the disease, but never occurred to me afterwards; nor did I feel, in the whole subsequent course of the malady, the smallest tendency even to nausea.

“ Plague is very generally supposed to be accompanied by acute pain, and feelings of excessive distress. This, however, I have not observed to be the case; but, on the contrary, for a disease so fatal, it has appeared to me peculiarly exempt from painful and violent struggles. It is true, that when respiration is materially affected, there is much moaning, and other symptoms of distress. When inflammation of the buboes and carbuncles is at the height, there must be, as in all acute inflammations, a good deal of pain. But, upon the whole, I cannot say that I have met with any instance of this disease, in which there has been so much racking torment, as I have sometimes witnessed, in other maladies, attended with fever and inflammation. Plague rather seems to take its victims, as it were, by the shoulders, and to shake them gradually out of existence.

“ With respect to buboes and carbuncles, considering them entirely as subordinate affections of the glands, skin, membranes, &c. which, when they do occur, follow the course of the general disease, I have left them purposely to the last. These phænomena appear, in various patients, in various situations, in various combinations, at

various periods of the disease ; and, in some, they do not appear at all. In some cases there are buboes, without carbuncles ; in some, carbuncles, without buboes ; in some, both ; and, in some, petechiæ, without either. The most common seat of buboes is in the upper part of the thighs, and in the arm-pits ; but they may take place wherever there are glands, and I have seen one in the pelvis. The most common seat of carbuncles is in the legs and feet ; but I have known them to occur in the breast, as in the case of Missail, of Duncapé, and in the arm, as in the case of Captain Spiraki, of Cephalonia.

“ To consider these collateral and subordinate affections, then, as distinguishing characteristics of the malady, appears to be an error no less general, and gross, than it is injurious. They may, or they may not, accompany the disease. But, when they do, they are merely appendages, as phlegmons are of common fevers. Their particular treatment cannot tend to remove, any more than their neglect to aggravate, the general disease. But the ignorant Papas, and illiterate Turkish surgeons, who engross the care of the hospitals, into which pestiferous persons are sent to die, in the Levant, in endeavouring to give themselves a consequence, by the reputation of experience and knowledge, have found these buboes and carbuncles very convenient instru-

ments for their purpose. If one patient, in twenty or thirty, happens to survive the general disease, they poultice, and cut, and plaster these local affections, and contrive to persuade the unfortunate sufferer, that they absolutely constitute the disease, and that, upon the treatment of them, although he is already recovered, depends his cure. Thus, those Charlatans, by bringing to suppuration tumours that ought to be dispersed, and, in their ulcerated state, making the patient go through a long course of what they call "medication," do actually contrive, since people of science keep aloof from the disease, to raise themselves, in the public opinion, to the rank of men of knowledge and experience; and they are even consulted, as such, respecting the general treatment of the malady.

"Let me now endeavour to give your Excellency a concise idea of what my plan of treatment *was*, under the circumstances, in which I have been placed, as well as of what it *would have been*, had those circumstances been different. According to certain medical principles, to which I have already slightly referred, the rule, for the application of curative means, is derived from a knowledge of the action of different powers upon different organs. Each power is known to act with a preference upon some particular organ; and when that organ is diseased, the power so acting

is the remedy. But there are many powers, which act with a preference upon the same organ ; and hence, we are supplied with a choice of remedies. For the sake of brevity, I shall confine my illustration of this principle entirely to plague. In that disease, I have considered the vascular, or circulating system, and the brain and nerves, as being the organs principally affected. The first question, with respect to the treatment, then becomes :—“ What are the powers, which act most appropriately, and with most intensity, upon these organs ?” Of this description, the *Materia Medica* affords many. *Digitalis*, calomel, and other powers, for instance, act with a preference upon the circulating system ; opium, and other agents of a like nature, upon the brain and nerves. But as both sets of organs are here affected, it is necessary to combine both sets of remedies. On this principle, I administered calomel and opium conjointly ; and, although I was obliged to relinquish more than one half of the efficacy of my plan of treatment, by abstaining from night prescription, as has been elsewhere explained, even the very deficient means, to which I was thus limited, proved, in two very dangerous, and several slighter cases, decidedly successful. Tumas of Roumelia, who, at his entrance, had been six days ill, and was covered with petechiæ, his pulse not to be counted, and with the symptoms of alie-

nation which have been described as proper to the malady, was dismissed, cured, in six days from his admission. Missail, of Duncapé, who had been three days ill, with a very extensive carbuncle on the right breast, was treated in the same manner. In five days, the sound separated, in a circle, from the unsound part of his breast: he was, in all other respects, doing well; and when I left the hospital, ten days after his admission, was in an advanced state of convalescence. The same treatment, in an inferior degree, according to their respective situations, was applied to Christophe of Cephalonia, and Nicholai of Carfania, who were both dismissed, cured, on the 23d of August; and to Andria of Cyprus, who was left convalescent in the hospital.

“ The treatment of my own case was guided by the following considerations. Reflecting that, if I attempted to take powerful remedies, repeated regularly, according to the duration of the action of each dose, in order to effect a speedy cure, as I had no person, not even a servant¹, whom I could entrust with their administration, in case of my falling into delirium, my constitution might irretrievably suffer, from the sudden discontinuance of the remedies themselves; I thought it

¹ My servant, as has been already stated, had absconded at the moment that I was setting out for the hospital.

more expedient to prefer a gentler, although a slower, and a less scientific process. It seemed most prudent, under the circumstances, to adapt the remedies to the exigencies of each day, even if the disease should be thereby considerably protracted. And, to this course, I was the more especially determined, by a persuasion, that my only chance of safety, in that place, lay in the preservation of my strength, and of my senses.

“ Having, on the first day, given a check to the disease, by some stout doses of calomel and opium, in the evening I bathed my legs in a mixture, consisting of an ounce of concentrated nitric acid, and about two gallons of hot water, for a quarter of an hour. Two hours afterwards (about eleven o'clock) I took a draught, consisting of a drachm of laudanum, the same quantity of tincture of litta, and half a drachm of antimonial wine, in some water; and, in the course of the night, repeated this dose. These remedies preserved me in a tranquil state, and produced a gentle perspiration, and some sleep. This mode of treatment I continued for several days, deviating, less or more, from uniformity, according to circumstances; always endeavouring, in the day, to keep my mind actively engaged, and walking out very frequently, indeed almost constantly, under the shade of some walnut-trees, in an adjoining burying-ground. In a week, or less, I

considered the disease as conquered ; and never, during its whole course, abandoned myself to bed, excepting for the second day, and some part of the fourth day, when my languor and indisposition were at the height. By these means, I prevented the increase of muscular debility, particularly of the legs, and supported my spirits, and general strength, in a surprising degree. The advantages, almost incalculable, of at once amusing the mind, and enjoying the benefits, of air and exercise, are all forfeited by the sick in general, in this malady, who, for the most part, throw a blanket over their heads, and give themselves up to languor and despair.

“ I sometimes took between three and four grains of plain opium, instead of the draughts at night ; but it had not the same tranquillizing and diaphoretic effects, and I returned again to the draughts. I also occasionally took one drachm doses of tincture of litta alone, which proved palatable, and appeared to be appropriate in their effects. The good effects of this remedy I attributed to its action upon the membranes, considering those of the thorax, and other parts, as partaking of the malady. I always continued to take some calomel and opium, or calomel alone, or opium alone, through the day, seldom exceeding four grains of the former, and two of the latter, in divided doses. The manner and proportion, in which they were

taken, were determined by my judgment of the state of the various organs for the time being.

“ But what may appear most singular, in this recital, is the very great gratification, no less than benefit, which I received, from the use of weak brandy and water, as drink, especially during the night. Its salutary effects were so conspicuous, that it seemed to be an agent the most appropriate possible to my then state. From this beverage, I did not experience the smallest effect in the way of intoxication. A few days afterwards, my relish for this liquor entirely ceased, and it even became disagreeable to me. The wines then took their turn—Sherry, Madeira, and Dardanelles, white and red; and I generally found that my taste was a pretty correct criterion of what kind was most fitting. The relish for the same wine did not continue long, seldom above four or five days. In this respect, something might have depended upon the remedies I was taking at the time; but, on this occasion, I cannot pretend to say that I was able, with any accuracy, to trace the connection.

“ The use of brandy and water was first suggested, as a drink, not in consequence of any reasoning, but merely by the circumstance of my friend, Mr. Black, of Pera, having considerably sent me a bottle of excellent brandy, along with some other good things, to the hospital, whic

happened to arrive at the very moment of my being seized with the plague, and to my finding it, with water, a very grateful beverage. The nature of the disease, however, seems to encourage the idea that such stimulants, although not adequate as remedies, may be appropriate as drinks: and I have been informed that the natives of the Levant, as I have also seen occasionally practised by individuals in the Greek hospital, are accustomed to administer, both in the plague, and for its prevention, considerable quantities of a spirituous liquor, which they call *Rackey*. But, in their hands, as might be expected, although it is of some utility as a palliative, it is of little permanent benefit; and, in the regular treatment of severe cases of disease, such inadequate agents ought only to be employed as occasional auxiliaries.

“ The most marked symptom, which I experienced, on the first day of the disease, was a vomiting, which, however, only continued for a moment, and never afterwards recurred. On the morning of the second day, I discovered a bubo in each groin. During the whole course of the disease, my bowels were kept free. My pulse was very variable, and small; but it seldom exceeded 108, or fell below 90. Ever since quitting the Greek hospital, on the 31st ult. being the twelfth day of the malady, i. e. since I

have been able to procure any thing fit to eat, I have lived uniformly on boiled fowl, eating nearly a whole chicken every day for dinner, and drinking moderately of wine, with a progressive improvement in health, being at this moment (18th September) as well as at any period of my life. I have not, during any part of the time, experienced much inclination for fruit, or a strong desire for any particular article of food; but I now begin to find some addition to the boiled chicken necessary.

“ From the occasional sketches, which I have here given, your Excellency, accustomed, as a philosopher, to contemplate medical subjects, will readily perceive, that my professional opinions are not those which are commonly taught in our universities, or schools of medicine. In some manuscripts, now at the palace, I have endeavoured to embody these opinions, and to reduce them to some degree of systematic arrangement. That I was not, before entering upon my investigation, more communicative respecting doctrines, which, upon the whole, may be found peculiar, I beg your Excellency to believe, arose entirely from an apprehension of being obtrusive, on a subject, which, I am aware, is, in its very nature, sufficiently repulsive, and, to persons not of the medical profession, apt to prove tiresome, or disgusting.

“ It now only remains for me to state, in a precise form, the results of my proceedings at the Greek hospital, from the 15th to the 31st of August.

“ Of fifteen patients, whom I treated, during that period, myself included, *six* recovered, reckoning the two convalescents, and *nine* died¹.

“ Of the plague patients, sent to the hospital, it is to be observed, that they were not, at least during the period of my residence, of a mixed description, as usually happens in pestilences. They were all at the last extremity, as if the town had been ransacked, to pick out every lost case that could be found. Not one was brought in, during the incipient stage of the malady. This might, it is true, have resulted from the nature of the institution, to which, considering it but as an intermediate stage to the grave, few will choose to consign a relative, until his recovery be totally despaired of; or, which is still more probable, it being quite consistent with the object of the intriguers concerned, from its being contrived, that, whilst all the lost cases were sent to the hospital at Constantinople, the milder and incipient ones should be sent to that at Pera, for the purpose of appearing to give to my experiments unfavourable results. Be this, however, as it may, it is quite obvious,

¹ See the annexed return.

that such a collection of patients, as is described in my return, is much less capable of being cured, than the general run of the sick, even in the height of the most violent pestilence.

“ But in order to form a correct judgment of their state, it is necessary to enter into a short analysis. Of those that recovered, some description has already been given. Out of the nine patients that died, two, Vimach, of Aptivisu, and Constantine, of Roumelia, have been supposed to have had their deaths accelerated by unfair means. Two others, Nichola, of Constantinople, and Anastas of Natolia, were within a few hours of death, when admitted; and three were so far exhausted that I did not think of prescribing for them with any curative view; viz. Demetrius, of Roumelia, Kiro, an old man, and a young girl of ten years. The only two patients, then, that can, with strict justice, be considered mine, among those who died, are Captain Spiraki, of Cephalonia, and Petré, of the Morea. The former had been twelve days ill; was seven days without a motion, and his pulse not to be counted. Whether these, or either of them, could have been saved, by a more correct application of remedial means, than was practicable, at the hospital, I will not positively affirm. But, at any rate, their treatment

became still more deficient, in consequence of my illness.

“ Here, then, it appears, that the cures performed, at the Greek Hospital, under all the disadvantageous circumstances, that have been described, amount to *forty per cent.* upon the whole number of the sick ; a proportion, which, under circumstances so peculiarly unfavourable, cannot be justly deemed but as denoting considerable success ; especially when we reflect how much smaller a proportion, in ordinary situations, usually survives, often not exceeding *three*, or at the utmost *four per cent.* In the Lazaretto Hospital, at Malta, for instance, toward the termination of the plague, in 1813, not more than *two* survived, as I have been informed by Mr. Grieves, Deputy Inspector of Hospitals, out of *between eighty and ninety* sick, or *little more than two per cent.* During the progress of that malady, the proportion that survived, throughout the island generally, has been estimated, by the President of the College of Physicians, (Proto-Medico), from actual returns, at *ten per cent.* As the disease became milder, the proportion of recoveries, of course, increased, but it never, at the very mildest period, exceeded, according to the same returns, *forty per cent*’.

’ Answers of the Proto-Medico to my Queries. Ans. 11, p. 27.

“ I have only farther to say, on this subject, that, my health being now re-established, I am perfectly ready to recommence, *in a proper manner*, the investigation of the plague: and that I remain confident of success. That is, I will undertake, with a well organized establishment, to save the lives of *sixty per cent.* of the ordinary run of pestiferous patients. By ‘a well organized establishment,’ I mean a building suitable to the object, with two assistant physicians, one surgeon, one apothecary, one or two interpreters, and a proportionate number of inferior agents all at my disposal and command. To make the essay on a smaller scale, would be no proportionate diminution of the expence. If either the Ottoman government, then, or the powers of Europe generally, should be disposed, on this principle, to patronize so important an undertaking, I am willing, in a rational way, to devote my whole time, and whatever talents I possess, to its advancement¹.

“ I have the honour to remain,

“ Your Excellency’s most faithful and

“ Obedient humble Servant.”

¹ The reader will please to observe, that, it was some time after I had made this communication to Sir Robert Liston, and after my decided intention, of recommencing the investigation of the plague, was known to all the friends, who

“ P. S. Reports having, I find, been circulated, in this place, that I used to plunge my patients in iced baths, and that I inoculated myself for the plague, with other extravagancies, no doubt, of the like nature, I think it right, although I have no apprehension that they will obtain credit with your Excellency, to take this opportunity of declaring, that they are totally destitute of foundation. With respect to a bath indeed, which I should have been much disposed to employ as a remedy, it was a luxury, which it would have been impossible to procure, in any shape, at this hospital, so invincible were the obstinacy and laziness of its inhabitants.”

visited me, at my quarantine quarters at Pera, that the Levant Correspondents of the Frankfort Journal, were at such pains to assure the public, that I had relinquished all such intention. See the reference to the article in the Frankfort Journal, and to its contradiction, in a subsequent chapter.

Return of Plague Patients attended by Dr. Charles Maclean, at the Greek Pest Hospital, near the Seven Towers, at Constantinople, from the 15th to the 31st of August, 1815.

<i>Name.</i>	<i>Date of Admission.</i>	<i>Principal Symptoms.</i>	<i>Event.</i>
1. <i>Andria</i> , of <i>Cyprus</i> ,	Aug. 3,	Pulse 120. Tongue glossy, but moist. Two buboes in the groins, and one carbuncle,	Convalescent 31st August.
2. <i>Christophe</i> , of <i>Cephalonia</i> ,	July 15,	Pulse 120. Tongue glossy, and moist. Bubo and carbuncle,	Cured Aug. 23.
3. <i>Nicolai</i> , of <i>Carfania</i> ,	July 26,	Pulse 96. Bubo and carbuncle,	Cured Aug. 23.
4. <i>Vimach</i> , of <i>Aptivisu</i> ,	August 15, 5 days ill,	Pulse 128. Tongue white, frothy, and very dry. Bubo left groin,	Died Aug. 16.
5. <i>Demetrius</i> , of <i>Roumelia</i> ,	July 20,	Pulse not to be counted. Tongue dry. Very large bubo in right groin. Cannot eat or speak. Emaciated to the greatest degree,	Died Aug. 27, 39th day of malady.
6. <i>Tumus</i> , of <i>Roumelia</i> ,	August 17, 6 days ill,	Pulse not to be counted. Tongue covered with a thick whitish crust. Petechia. Convulsive appearance of countenance,	Cured Aug. 23.
7. <i>Constantine</i> , of <i>Roumelia</i> ,	August 17, 4 days ill,	Pulse 108. Tongue very white and foul. Usual convulsive aspect. Bubo and carbuncle right side,	Died Aug. 18.
8. <i>Anastas</i> , of <i>Natolia</i> ,	August 19, 4 days ill,	Pulse 132. Tongue red and dry. Bubo with great pain in the right axilla,	Died Aug. 20.
9. <i>Missail</i> , of <i>Duncapé</i> ,	August 20, 3 days ill,	Pulse 112. Tongue white and foul. Large carbuncle right breast,	Convalescent August 31.
10. <i>Petré</i> , of the <i>Morea</i> ,	August 22, 4 days ill,	Bubo above the pubes, in the pelvis, right side. Much heaving of the breast. Pulse 120. Tongue white. Head ach,	Died Aug. 24.
11. <i>A Girl</i> 10 years old,	August 26, 9 days ill,	Eyes glazed and sunk. Pulse cannot be counted. Tongue dry and furred. Moans much. Carbuncle under right knee,	Died Aug. 30.
12. <i>Capt. Spiraki</i> , of <i>Cephalonia</i> ,	August 27, 12 days ill,	Pulse not to be counted. Seven days constipated, angry carbuncle right arm, close to elbow,	Died Aug. 29.
13. <i>Nichola</i> , <i>Mason</i> , <i>Constantinople</i> ,	August 28, 5 days ill,	Pulse not to be counted. Tongue red and dry, with a white scurf,	Died Aug. 29.
1. <i>Kiro</i> , an old man,	Aug. 26, could get no account of his illness for want of interpretation,	Pulse 120. Tongue dry and hard. Bubo in right groin, and carbuncle under right knee. Exhausted.	Died Aug. 28.

N. B. A lad, called *Apostoli*, who was brought to the hospital just in time to die in it, is not, of course, included in the return, not having been under any medical treatment; nor the boy, who was admitted, and sent away, on the day that I was confined to bed.

The copy, which I sent to Mr. Morier, was accompanied by the following letter.

Pera, 30th Sept. 1815.

My dear Sir,

I consider it my duty to transmit to you, for the information of the Levant Company, a report of my proceedings, in this country, for the investigation of the Plague ; and perhaps I cannot do this better, or more fully, than by transcribing that, which I have prepared for his Excellency, the Ambassador, for the information of the British Government.

I cannot suffer myself to omit this opportunity of requesting you, individually, to accept of my most cordial thanks, for the many civilities which I have experienced from you, in the course of my residence here, and more especially for your uniform kindness and attention, during my illness, and subsequent quarantine.

I remain, with sentiments of the greatest respect and esteem,

My dear Sir,

Your's most faithfully and sincerely.

*To Isaac Morier, Esq.
Consul General, &c. &c. &c.*

CHAP. XXXI.

Reasons for not making the experiment of inoculation—attacked with cynanche maligna—its cause.—Accounts of the death of my Mother—visited by Mr. Frere, Mr. Lindsay, and Mr. Shoolbred—released from rigid quarantine on the fortieth day of the disease—use the Turkish Bath, and remove into fresh quarters.—Visit an Armenian child.—Anecdote of Mr. Barboux.—Eccentricities of Colonel Rooke.—Correspondence with Sir Robert Liston and Mr. Pisani—negociations with the Porte and the Greeks—misrepresentation of the Journal de Frankfort, and its contradiction.—Rejection of my propositions.—Invitation of Lady H. L. Stanhope, from Mount Lebanon.

NOTWITHSTANDING the confidence with which it has been repeatedly asserted, it never entered my mind, to have recourse to a measure so superfluous, misleading, and unscientific, as inoculation with matter from a pestilential sore ; for this plain reason, among many others, that, as plague is universally acknowledged to be capable of affecting the same person repeatedly, inoculation, if the disease depended upon contagion, instead of preventing its recurrence, could only do mischief, by producing it ; and, if it did not, could serve no purpose. Hence such an operation would be at least superfluous.

But it would be also misleading. The operations of nature cannot be so obscure, that, where contagion exists, such means can be necessary to set it in evidence. It did not remain doubtful whether small pox depended upon contagion, until the process of inoculation had been adopted. Let us, then, enquire what can be proposed as the results of such an experiment in plague. We shall suppose that the disease does, and that it does not depend upon contagion; and enquire, what, under each of these suppositions, would be the inferences supplied by the results of inoculation. Matter from a pestilential sore is inserted in a sound person. He either takes, or does not take the disease. In the former case, if it should *not* depend upon contagion, the conclusion afforded by the result of the experiment would be fallacious: in the latter, if it *should* depend upon contagion (for we know it to be consistent with the laws of life that diseases, which are notoriously contagious, may, in their progress, be attended with abscesses, or sores, the matter from which will not produce infection) the conclusion afforded by the result of the experiment would be equally erroneous. Here, then, we have a double source of fallacy.

But such experiments, if they were necessary, or could be efficient, are so difficult, under the existence of the belief in contagion, that, in

plague, they cannot be made in such numbers as to supply sufficient data for broad and general conclusions. The few attempts, which have transpired, as far as they incidentally furnish any inference, are positively unfavourable to the hypothesis of contagion.

Upon the whole, I have considered the inferences supplied by such experiments, even if we could depend upon the accuracy, with which they are related, as so liable to fallacy, that, whilst they seemed to favour my doctrines, I have not hesitated to reject their aid. And I will venture to predict, that those, who may continue to rely upon them, in the present state of opinions, and of pest institutions, will reap nothing but disaster to themselves, and disappointment to the public¹.

To depend upon an experiment, which cannot be conducted on a large scale, and, which, conducted upon any scale, is liable to so much fallacy, when we have facts the most abundant, and data the most irrefragable, by which the question may be decided, upon the broadest basis, would, as it appears to me, be at least a work of supererogation.

¹ The fallacy, to which such experiments are liable, will be further illustrated, by the researches, and catastrophe, of Drs. Whyte, Valli, and Mr. Von Rosenfeldt, as related in Chapters xxxiv, xxxv, and xxxvi.

Such are my reasons for having chosen to reject an experiment, which, from my particular view of the subject, I could not have regarded but as, in itself, perfectly harmless. No one, I should think, will suspect me of having declined it on account of apprehension of danger from the disease; since it is notorious, that, during my residence at the Pest Hospital, I visited, at regular intervals, six times each day, every pestiferous patient in the house; placing myself in contact with them, as is usually done, in other places, with respect to ordinary patients; preparing and administering their medicines, with my own hands; and watching those of them about whom I was most anxious, in a chamber adjacent to my apartment. For these proceedings, I do not claim any particular credit. Every person, I presume, entertaining similar opinions, and having a similar confidence in his knowledge of the means of cure, would have been equally ready to have acted in a similar manner. Nor should I here have noticed these incidents, but in order, in correcting mis-statements, the more fully to explain the motives of my conduct; wishing equally, in an investigation at once so delicate and important, to protect my own proceedings from misrepresentation, and to guard the public against delusion.

Wednesday, Sept. 20th. For the last ten

days, kept no regular Journal of my health. Have, in the course of the week, been attacked with an affection, which, by some, would be called a relapse, and by others, a malignant cynanche. The inflammation of membranes extended from the throat towards the stomach, and I experienced both considerable pain and difficulty in swallowing. The weather began to get cold ; and, the house being exposed, and full of windows, without glass, the wind and rain passed through it, as through a sieve.

Under the actual state of the buildings of Constantinople, the wonder is not, that there should be so much sickness, but that any one should remain well. By the blowing of the winds in currents, through every cranny, a degree of cold is produced, or the heat of the body is diminished, so as to occasion disease. I doubt whether even houses destitute of windows, if they had any sort of ventilation vertically, would not constitute less unwholesome dwellings than those which now exist. The noxious influence would then consist in a regulated and moderate diminution of the pureness of the atmosphere ; and would not be likely to produce diseases of such intensity, as the operation of its more sudden and violent transitions. To these transitions I attributed my new malady ; which, however, was removed, by ambulating blisters, and other means.

Received a letter from Mrs. Maclean, announcing the death of my worthy mother. Excellent woman ! she might have bid adieu to the world without regret ; for she had left few duties unperformed in it. She was in her seventieth year.

About this period was favoured with a visit from Mr. Frere, the Secretary of Legation, and the Rev. Mr. Lindsay, Chaplain to the Embassy, who had returned to Pera, from an excursion in the country. Mr. Shoolbred, who had just arrived from Smyrna, came also to see me.

On the 25th, wrote to Mrs. Maclean, mentioning the probability of my speedy return to England, having perceived, that, as there was from the beginning, no real desire to promote the investigation, in which I was employed, there was now less appearance than ever of its being resumed, upon an amended plan.

Friday, Sept. 29th. This being the fortieth day from my being attacked with the plague, I was presumed to be in a state admitting of a nearer approach to social intercourse, without danger to the community. But it was first deemed necessary that I should perform ablution at a Turkish bath. I accordingly proceeded to a house on the confines of Pera, where, accompanied by Don Courban, who was master of the ceremonies upon the occasion, and used the bath along with me, I performed this purification, as Dr. Johnson

says of Swift, "with oriental scrupulosity." It was an affair of great solemnity and form. It was necessary that I should be habited anew, from the crown of the head to the sole of the foot. A trunk of clothes, which had been left at the British Palace, when I entered the Pest Hospital, was sent before me to the bath-house, containing every thing that was necessary, excepting a new hat, which was purchased for the occasion. Being thus equipped, it was necessary that the baggage, which had been at the hospital, and in quarantine, should be properly disposed of. The papers I would not lose sight of. They were therefore fumigated in my presence, and I took them along with me. The linen was left to be washed and purified at Don Courban's. The clothes, which I wore among the pestiferous patients, had been some time in the possession, and on the person of Francisco, who daily perambulated the streets of Pera in them; and it was not thought necessary that either the dress, or the wearer, should undergo any purification. But what amused me the most, amongst the inconsistencies of these proceedings, was to find, that an old great coat, and a close coat, pantaloons, a hat, and other articles, which Francisco conceived to be his perquisites, of right, should have created some jealousy in the Priest, who came, the following day to inform me that, conceiving Francisco had

wished improperly to appropriate these articles, which he considered much too good for him, he had withheld them from him, evidently expecting that I would make a transfer of them to himself; and he was obviously much mortified and disappointed, when I informed him that Francisco had my free permission to take possession of these articles. These circumstances I have deemed it proper thus minutely to relate, in order to shew that persons, like Don Courban, who are regarded as authorities, in respect to matters of plague police, either do not themselves seriously regard cloathing as capable of communicating infection; or, that, when a little matter of property is concerned, they are wholly indifferent to the safety of the public.

Purification of person, and purification of goods, being thus duly performed, I proceeded from the apartments of Don Courban, at the French pest house, to apartments which had been previously hired for me, by Mr. Pisani, at the house of Madame Chatain, a French woman, in the street leading from the British Palace past the Turkish Cadet School, to the usual place, of embarkation, at Tophana. I am obliged to employ this circumlocution, because, in Constantinople, the streets have no names. In order to find any inhabitant, he must be addressed to the north, south, east, or west of the Mosque,

Chaan, Bazaar, or other public building, which is nearest to his residence. Neither are the houses numbered.

On the 30th, in consequence of the urgent solicitations, or rather importunities of an Armenian merchant, seconded by the entreaties of my Turkish language-master, to whom I could not refuse so small a sacrifice, I consented, although I had determined not to engage in general practice, to visit a sick child, at the distance of several miles from Pera, up the canal of the Bosphorus. It was towards evening. The child had been labouring under a diarrhœa, of thirty-six days standing, which was now converted into dysentery. It was attended by four persons called doctors, and the afflicted parents shewed me some pint bottles of oleaginous mixtures, directed to be given in spoonfuls, which would have been overpowering for the stomach of a young bear. It was actually in *articulo mortis*, and in an hour afterwards expired. In consequence of this journey, I caught a cold, which was troublesome for some days, but yielded without much medical treatment.

Here, in my new habitation, I was still in an intermediate state of existence, a sort of purgatory, neither absolutely in quarantine, nor completely restored to society. From deference to the prejudices of my friends, I abstained from

paying visits, until I was assured that they had ceased to entertain apprehensions of being infected. They ventured, by degrees, successively to enter my dwelling, to sit down in the same room, and even on the same couch with me; and I, of course, did not delay to return their visits. I was now pressed to undertake the treatment of patients; but declined it, referring the applicants to Dr. Macguffog, as a gentleman properly qualified, and intending to take up his residence among them. Mr. Barbaux, who had resided between thirty and forty years in Turkey, was the first to ask me to dinner. He seemed to entertain no dread of contagion. In the course of conversation, he mentioned a very striking circumstance, relating to this subject, which took place soon after his arrival in the Levant. Proceeding in a Russian vessel, from Constantinople to Odessa, one of the passengers, a Russian officer, was so ill, that he requested to be landed at Buyukderé. On the landing of this gentleman, the nature of whose complaint was not known to those on board, Mr. Barbaux requested that he might succeed to his birth, which was better than his own; and, in doing so, he succeeded also to his sheets and his night-cap. Upon approaching their place of destination, the people on board this vessel were astonished to find themselves fired at from the shore, and peremptorily

ordered to stand off. Upon enquiring into the reason, they were asked if such and such was not the name of the vessel, and if she had not had a sick passenger on board, who had been landed at Buyukderé? The answer being in the affirmative, they were informed from the shore, that the person in question had died of the plague on the day subsequent to his being landed, and that the Russian Legation had immediately dispatched a courier from Buyukderé to Odessa, with intelligence of the event, warning the governor against suffering the vessel to approach. It is scarcely necessary to add, that the sheets and the night-cap did not prove injurious to Mr. Barbaux.

The following traits of eccentricity were related to me, on this occasion, by Mr. Barbaux. In the year 1814, there died, at Cyprus, at the age of seventy, an Englishman, or Irishman, of singular taste, and eccentric habits. His name was Rooke. Colonel Rooke had passed the last twenty years of his life almost entirely amongst the islands of the

¹ I have been informed, that, an attached servant, who had been many years with Mr. Barbaux, was lately seized with the plague, and died; and that his humane master, instead of consigning him to inevitable destruction, by sending him to any of the pest depôts, took lodgings for him, and used to visit him in person. Traits like these deserve to be recorded and eulogized.

Archipelago. He had a yacht, handsomely fitted up, constantly employed to carry him from island to island, as inclination or caprice might suggest. His manner was to take up his residence, whilst ashore, in one of the convents, with which the Greek Islands abound. Thither he would cause plate, furniture, wine, and provisions, to be conveyed from the vessel, according to the stay which he intended to make; always, at his departure, remunerating the Papas liberally, for the use and accommodation of their convent. He was supposed to possess an income, either in England or Ireland, of from three to four thousand pounds a year. He never came to Constantinople, or Smyrna, but for his remittances, which generally happened at two seasons of the year. On some of these occasions, he used to transact business with Mr. Barbaux. There was one island, (one of the obscurest, of which I do not recollect the name) to which he was partial, and made his head quarters. Here he purchased a garden of which he gave the property to a native woman, on condition of her keeping it in order, reserving to himself the fruit.

It was on Friday, the 6th of October, that I dined with Mr. Barbaux, which I considered the first day of my free admission into society. On the fifth I had written to Sir Robert Liston, stating, in general terms, my determination, as soon as I

should receive the final answer of the Porte and the Greeks, if, as I had now reason to believe, it should be unfavourable, to return to England; and to Mr. Pisani, that I was only waiting for that decision.

From Mr. Pisani, with whom the whole conduct of the negociation, on my side, rested, I immediately received the following answer:

“My dear Sir,

“I do not lose sight of your affairs one instant; but, unluckily I do not meet the same diligence on the other side. I am not yet ready for an official answer. I shall go over to-morrow, when most probably the point will be finally decided. I remain, with great attachment and esteem, &c.”

Immediately upon the receipt of this note, I proceeded to Mr. Pisani's house; and, in a conference, which took place between us, he being stationed at the top, and I at the bottom of a long staircase, he began by observing, that it was understood at the Porte, it was not my wish to resume my investigation of the plague. I replied, that I was at a loss to conceive upon what grounds such an understanding could have gone abroad; that it was a rumour wholly unauthorized by me; and, that, on the contrary, it would be seen, that, if the investigation were not forth-

with resumed, the fault should not be mine. He still was, or affected to be, incredulous; and, observing that I might freely declare to him what I might not choose explicitly to state to Sir Robert Liston, requested that I would, as to a friend, frankly tell him my mind upon the subject. My answer was to this effect. "I have no mental reservations, Mr. Pisani. My declarations to Sir Robert Liston, to you, and to every other person, upon the subject of resuming the investigation of the plague, have been uniform, and the same. I am ready to recommence it to-morrow: and if that opportunity should not be afforded me, the fault will lay entirely with the Porte and the Greeks. But, lest any misunderstanding should take place, from communications merely verbal, I shall, to-morrow morning, transmit my sentiments to you in writing." He then expatiated upon the expence, according to the extensive scale of operations which I had proposed. I replied, that, in order to remove every obstacle, as far as depended upon me, I was willing to reduce that scale. He asked what I supposed the expence might be of a limited establishment? This was a question, which it was impossible for me, with any accuracy, to answer. But, I presumed, as I wished for nothing for myself but a house and table, upon a very moderate scale, the whole expence of the proposed establishment,

even if a new one were to be instituted by the Porte, need not exceed two thousand pounds a year. But, if the expence were the real and only obstacle, I should even not object to the resumption of my labours at the Greek hospital, provided I had a separate place to live in, a proper table was kept, and the servants were allowed to be chosen by myself, and to be dismissed, or replaced, as I might deem proper, or necessary.

Throughout the whole of this conversation, I could easily perceive, that the rejection of my plan in toto, excepting in a situation in which I could not ensure my personal safety, had been already determined upon at the Porte ; and that the pretended negociation had no other object than that of eluding the responsibility of a direct refusal. I was however resolved, as the subject concerned all nations, and not only the present generation of mankind, but posterity, that the refusal, if possible, should be both formal and regular.

It is not a little remarkable, that an article addressed from Constantinople, to the Frankfort Journal, on the 25th of September, as if in unison with the politics of the Porte, in respect to my investigation, takes some pains to assure the public, that I had entirely renounced the desire of continuing my experiments. From Vienna, I

afterwards noticed, and contradicted this paragraph in the following terms, addressed to the Frankfort, and other German Journals :—

A L'ÉDITEUR, &c.

Vienne, 4 Décembre, 1815.

Monsieur,

A mon arrivée ici, il y a peu de jours, je lus dans votre numéro du 11 Novembre l'article suivant, sous la date de *Constantinople, le 25 Septembre* :

“ Le medecin Anglais Macléan, qui prétendait antérieurement que la contagion n'était pas très à craindre, en a été lui-même attaqué peu de jours après avoir commencé à donner ses soins aux pestiférés de ces lazarets. Il est presque en convalescence, *mais* on assure que, content de cette épreuve, il a entièrement renoncé au désir de continuer ses expériences.”

Loin de rénoncer à ce désir, je ne fus pas plutôt rétabli que j'offris formellement aux autorités constituées dans le Levant de recommencer mes recherches, à condition qu'on fit les frais d'un établissement adapté à cet usage, m'étant suffisamment convaincu que ce qu'on appelle les hôpitaux Grecs sont incompatibles avec les moyens de perfectionner le traitement de cette maladie, &c.

The Levant correspondents of the periodical press, in their representations of the cause of my malady, would of course adhere to the prevailing illusions; but respecting my future intentions, they could have learnt nothing from any authentic source; and, as the intelligence, which they did convey, was as contrary to truth, as it was in conformity with the views of the plague dealers, and the Porte, we are furnished with a pretty certain indication of the source of their knowledge.

On returning home from Mr. Barbaud's, I found a note from Mr. Pisani, dated that evening, in the following words:—"I have just received a letter from the Right Hon. Lady Hester L. Stanhope, of which the greatest part concerns you, my dear Sir, with whose undertaking and recovery I happened to have acquainted her Ladyship. I inclose it for your perusal. You may return it at your leisure."

In this letter, her Ladyship is pleased to say, that, if I should find it convenient to visit Mount Lebanon, she would communicate to me all the information, which she had collected, and which was considerable, respecting the plague, *if the object of my journey were really to investigate that malady.*

This is the import, I do not affect to give the precise words, for I did not take a copy of the

letter. The insinuation conveyed by the part printed in *Italics*, struck me as singular; and I could not account for it, in any other manner, than by supposing, that, as befel Dr. Valli in Asia Minor, or Syria, it is the fashion in Turkey to traduce every investigator of the plague, who has the good fortune to escape the disease, the bowl, and the bow-string, as spies, i. e. infamous reptiles, that ought to be swept from the face of the earth. Perhaps Lady H. L. Stanhope, if these pages should ever be so fortunate as to attract her notice, will condescend (nay, I am persuaded, she will think it due both to me and to herself) to favour me with some explanation, respecting the ground of her remark; for assuredly such an insinuation could never have originated with her Ladyship.

To an observation of this nature, under ordinary circumstances, I should not have been disposed to affix any particular importance; but there were occurrences which rendered it probable, that, in whatever manner, or from whatever quarter, the idea might have been conveyed to Lady Stanhope, that I could have been in the Levant for any other purpose than that which I professed, the report must have formed part of a regular system of vilification, which it has been usual to practice in that country, against inves-

tigators of the plague. To this purpose, I may mention, that, on the very day of my departure from Pera, meeting with Hogia Meckerditch, my Turkish language-master, he expressed his surprise *now* to find, as he said, that I had not, in this investigation, been accredited by the British government; adding, "how deceived we all were!" Being in a hurry, at the moment, and my mind being intent on other matters, it did not occur to me to enquire where he had got this new light, which must have rendered me so different a person, in his eyes, at the commencement, and at the termination, of my investigation. But, reflecting upon it afterwards, and recollecting that the French woman, in whose house lodgings had been procured for me at Pera, had several times introduced discourse, without any obvious connection, upon the subject of spies; as well as that Dr. Valli, had actually been treated as a spy, by one of the Turkish Pachas, in Asia Minor, or in Syria; it seemed to be an obvious inference, that a regular system of vilification, in respect to investigators of the plague, had been established in Turkey. But, these reflections were calculated to excite compassion, rather than anger. For, my experiments being completed, I could feel no apprehension, that their results, if they should remain,

for a time, unaccredited, or be even opposed, would not, by their intrinsic truth, disperse the fogs of prejudice, and finally prevail.

This is a new species of espionage. If the services expected from persons of that description were attended with as much danger, and as little profit, as that in which Dr. Valli and myself had been engaged, I may be allowed to question, whether the profession would have many followers.

Saturday, October 7th.—In returning Lady Stanhope's letter, this morning, I thought proper, in consequence of what passed yesterday verbally, to address a letter to Mr. Pisani in the following terms :—

Pera, 7th October, 1815.

“ My dear Sir

“ I am gratified by the notice of Lady H. L. Stanhope, which your kindness has procured for me, and, were it practicable, would have much pleasure in paying my respects to her Ladyship at Mount Lebanon. The opportunity of making her valuable acquaintance, will, I trust, occur hereafter.

“ From what passed between us yesterday in conversation, I think it right explicitly to declare

to you, that I have not, and never had, any idea. but of seriously prosecuting the investigation of the plague, if the means of doing so, in a proper manner, should be granted me ; and that I am not aware, that any conduct, or expressions of mine, could have authorised a different conclusion, Whatever may have happened to *others*, neither *my* zeal, nor *my* confidence, are diminished in the smallest degree, by the circumstance of my having caught the disease. I therefore request you, in my name, peremptorily to disavow all surmises of an opposite nature. With respect to my wish to return to England, previously to entering upon a permanent residence here, the explanation is quite natural, since Mrs. Maclean insists, in such a case, upon accompanying me to this country, and participating in the dangers of the investigation ; besides the impossibility of procuring elsewhere proper assistants, and remedies that can be depended upon, &c.

“ Having made this declaration, which, I trust, will enable you to remove any doubts that may have arisen upon the subject, I have only to add, that, if the gentlemen concerned in the negotiation respecting the hospital, are willing to proceed zealously and frankly in the business, I shall be satisfied, in the mean time, with an income sufficient to defray my expences in a becoming man-

ner, and a proper house to reside in ; and afterwards look for a reward proportionate to the services, which I may have rendered.

“ Please to present my compliments to Mr. Argiropulo, and tell him I am anxious to convince him, as I know he is capable of appreciating my views, that I wish to act upon the most liberal and just principles in this matter, without, however, forgetting what is due to myself.

“ I am, &c.”

“ To B. Pisani, Esq. &c. &c.”

On Saturday night, the 7th of October, I received the following note, dated that evening, from Mr. Pisani :

“ My dear Sir,

“ This serves to inform you, in two words, that all idea is given up of accepting the plan in question. To-morrow, or the day after, I shall repeat this in a more formal way.

“ I remain ever, &c.”

But it was not until Friday, the 13th of October, that the official decision of the Porte, antedated to the 7th, was communicated to me, ac-

accompanied by the following note from Mr. Pisani :—

" Friday Evening.

" My dear Sir,

" I enclose my official report, which I have consented to write, because you wished me to do so. Being, therefore, desirous to comply with your demand, I felt bound to ascertain and delineate the true sentiments of the Porte on the subject, being most truly and ever at your command, &c."

It must be obvious, that this official refusal was in a manner forced from the Porte, and the motive of their reluctance to commit themselves cannot but be evident to every reader.

" Pera, October 7th, 1815.

" Sir,

" Having spoken with the dragoman of the Porte, Mr. Argiropulo, who has been charged by his Excellency the Reis Efendi to treat the business of your undertaking from first to last, upon the subject of the offers made since your recovery from the disease ; he informed me this day, that, after holding several consultations with those who are best qualified to judge of things on a matter of this sort, he is sorry to have to say, that he has

nothing else to convey in answer, but the thanks of all persons concerned, for the disposition you still profess to continue in the pursuit, though upon a different scale from what has been tried before.

“ The funds allotted for such purposes, and with which three distinct establishments are kept up in different parts of the town, with no small expence, he observes, will not bear the additional charge of building a new hospital, nor hiring a house separate, and much less of making such an allowance as may enable you to try another experiment, by having an establishment under your exclusive care.

“ Besides this obstacle, the persons consulted in the case, represent, that, under your determination to trust to, and employ no other people, but such as may be of your own choosing, and under your immediate control, they can hardly conceive a possibility of seeing the present idea carried into effect to any purpose, when the plague happens to be truly serious, in which case there are often from one to five hundred patients lying with the infection at one period, all equally entitled to the humane assistance of those who are at the head of the department ; and that the objection, which now occurs, is the more natural, as it cannot be fairly said yet, that any certain proofs

have been obtained of the efficacy of your method, in the first trial that has been made.

“ These being the answer, and feelings, stated to me at the Porte, I have judged proper to convey them to you by letter, being, with great truth and regard,

“ Sir,

“ Your most obedient humble Servant,

“ B. PISANI.”

“ *Dr. Charles Maclean.*”

CHAP. XXXII.

Appreciation of the pretexts of the Porte for rejecting my propositions—considerations respecting the mode of my return to England—impossible to avoid another quarantine—determine to proceed by the Black Sea to Warná—prepare an itinerary of the route to Vienna—have permission to visit at the Ambassador's—again accredited a member of society—departure of Sir Robert and Lady Liston, for England—dine with Mr. Frere—meet with Mr. and Mrs. Rich, of Bagdad—take leave of Mr. D'Italinsky—confusion occasioned by my appearance at the Austrian Internuncios—obtain a passport and letters for Germany—letter from Mr. Pisani—from Dr. Kuttel of Pest, in Hungary.

THIS was much such a denouement as I had reason to expect. The pretexts set forth for rejecting my propositions, are, however, so entirely frivolous and unfounded, that, I confess they excited in me some degree of surprise. If the funds of the Greeks were inadequate to the expence of maintaining the establishment proposed, which however there is ground to believe was not the case, might they not, out of the three bad hospitals which now exist, have made one good one? Or, if the Greeks had had no

funds at all, ought not the Sublime Porte to have been ready and willing to defray the expence of an establishment for the medical treatment, and preservation of the lives of their Greek subjects? Most assuredly they would, had not the personal interests of the Grand Seignior been more advanced by their destruction. As to the remarks, that, in case of an extended pestilence, my plan would not be applicable, and that sufficient proof had not yet been afforded of the efficacy of my treatment, they are almost too ridiculous to require a comment. Surely a plan that is applicable upon a smaller, might be rendered applicable upon a larger scale, and with a proportionally extended benefit. And, with respect to medical treatment, as there usually existed none, either in the Pest Hospital of the Seven Towers, or in any other, mine, unless it were worse than useless, must have been an improvement. That my treatment of the sick might have been represented, by the persons who surrounded me, as worse than useless, or even as terribly destructive, I can readily believe. But I cannot believe the Sublime Porte so destitute of common sense as to have considered the testimony of the executive agents, or even of the directors of the Pest Hospital, in such a case, as worthy of the smallest credit; nay, I am well persuaded, that no person of sound understanding, will, for a moment, be-

lieve, that, unless the directors, and the servants of the hospital had been well assured, that unfavourable representations would have been agreeable to the Porte, they would have dared to have made them.

Upon the whole, a comparison of the contents of this letter, with the endeavours of the directors of the pest house, to induce me, at the very moment of my illness, to remain a month longer in that place, as well as the treatment which I experienced from the executive agents while there, afford, I must think, sufficient grounds for the conclusion, that the whole of the proceedings, upon this occasion, were rendered subservient to a policy, of which I narrowly escaped becoming the victim.

Here, the reflection will necessarily arise, how it could happen, that the Porte, the directors of the pest house, and the executive agents, should, at one time, all have been anxious, that, ill as I was, I should continue to reside in the hospital; whilst they, afterwards, upon my recovery, and offering to resume my labours, provided some new arrangements, easily practicable, and not very expensive, were adopted, wholly evaded, and, in the end, entirely declined acceding to my propositions? They still, let it be observed, continued very willing to receive me back into the hospital, trammelled as before; nay,

they continued even solicitous for my returning, having repeatedly caused it to be intimated to me, that the horse was still kept in the stable, waiting my orders, and desiring to know what I wished should be done with it. This conduct, and these proceedings admitted but of one interpretation. "You shall not be allowed an opportunity of continuing your experiments, in an establishment, which shall be wholly at your command, and in which you may act as you think proper. If you resume them at all, it must be in a situation, in which the Porte, the directors, and even the servants of the Pest Hospital, shall have the power, at whatever time, and in whatever manner, they may think fit, not only to thwart you, in all your proceedings, even to the destruction of your patients, but to starve, and otherwise maltreat you, and even to put an end, at whatever time, and in whatever manner, they may think fit, both to your investigation, and to yourself." Such was the construction that I most certainly did not hesitate to put upon the conduct of these persons: and, under such circumstances, it would have been the extreme of folly, or of madness, in me, to have afforded them an opportunity of carrying their designs into effect.

It will be observed, that, in this document, the word "infection," according to the meaning

given to that term by modern Christians, is employed. But the reader is requested to recollect, that it is Mr. Pisani, a Byzantine Catholic¹, who is communicating, in his own acceptation of them, or “delineating,” as he says, the sentiments of the Porte. No word signifying what is understood, amongst modern Christians, by *infection*, or *contagion*, as applied to epidemics, would have been applied by a Mahommedan.

This negociation being disposed of, I was now left at liberty to consider respecting the least exceptionable mode of returning to England. Here the baleful influence of the belief in contagion again intervened to disconcert me. Whether I should determine to proceed direct by sea, or first to Smyrna, to Malta, to Corfu, to Venice, to Naples, to Palermo, to Marseilles, to Leghorn, to Genoa, to Cadiz, or to Gibraltar, or, on the other hand, to Odessa, or, by land, through the Austrian dominions, a repetition of quarantine was every where staring me in the face. Under the surfeit, which I had just experienced of that species of discipline, the shorter duration

¹ Mr. B. Pisani, the senior dragoman of the British legation at Constantinople, who conducted this negociation, on my part, is a native of the Levant, descended from a highly respectable Italian family, and a gentleman of distinguished talents. In Lucien Bonaparte's *Charlemagne*, we find that the family of Pisani have given doges to Venice.

of quarantine, (the ordinary period on entering Austria, is eleven days, which is diminished, or increased according to circumstances) would have been alone sufficient to have determined me to prefer the route by land. But I had also another powerful motive for this choice. I hoped to have an opportunity of attracting the attention of the combined sovereigns to the results of my investigation, and of being able to induce them to cause my discoveries to be extensively applied, for the benefit of mankind.

The journey from Constantinople to Bucharest, by land, would, however, according to the mode of travelling in that country, have been too fatiguing for a person just recovered from the plague. I therefore, by the advice of my friends, who had experience of the route, resolved to go by the Black Sea to Warna. For this purpose, it was necessary to wait for a favourable wind, it having been blowing in an adverse direction for many weeks. These matters being definitively arranged in my mind, I resolved to employ the period of my farther detention at Constantinople, in visiting my friends, and collecting information necessary for the journey.

With the assistance of Mr. Serle, a member of the British factory, who had in his possession a very circumstantial Journal of the route, and of Mr. Frederick Pisani, one of the dragomen

to the embassy, who had himself recently travelled that road, I was enabled to prepare an itinerary, which I afterwards found of very great service, to Vienna.

The weather, in the course of October, becoming very cold, I had scarcely recovered from the quinsey, when I was again, notwithstanding every precaution, seized with a slighter species of sore throat, accompanied by catarrh, which, although not so severe as to require confinement, were troublesome.

Sir Robert and Lady Liston were now on the eve of departing for England. On the 6th, his Excellency addressed to me a friendly letter, in which he stated, that it was his intention to write, by the following post, to the Secretary of State for Foreign Affairs, in reply to the letter which he had received from that noble lord, respecting my mission; that he would send me a copy of his letter; and that I should be at liberty to make what use of it I might deem expedient. A few days afterwards, I had regular permission to visit at the Ambassador's: and, on the 12th, he embarked, with his lady and suite, on board a British frigate for Smyrna.

On the last evening, that I visited at Sir Robert Liston's, it was intimated to me, that the horse was still in the stable, at the Greek Pest Hospital, and I was asked what I should wish to be done

with him, as if an idea were still entertained that I might be disposed to resume my investigation, under circumstances similar to those in which I had previously been placed. I replied that they would dispose of the horse as they should think proper ; for, without a total change of system, such as I had indicated, I should never consent to enter the Pest Hospital of the Seven Towers, or any similar establishment.

I was now again accredited as a member restored to society. After the departure of Sir Robert Liston, I was repeatedly invited to dinner parties by Mr. Frere, who succeeded as Minister Plenipotentiary *ad interim*. Amongst the company, which I met with at his house, were Mr. Rich, the East India Company's Resident at Bagdad, and his lady : the former, well known for his literary taste, and attainments ; and the talents and accomplishments of the latter such as I should have expected in a daughter of Sir James Mackintosh.

Mr. Frere was good enough to apply to the Austrian Internuncio, with whom I had not the honour to be acquainted, for a passport, and letters of introduction. Mr. Pisani procured a firman from the Porte, with letters to the Prince of Wallachia, and the Greek Bishop of Warnæ : and by the 18th I was ready to depart. But the

wind, which had been so for many weeks, still continued adverse for crossing the Black Sea.

I availed myself of this interval to take leave of the venerable Mr. D'Italinsky, at Buyukderé. Not knowing the precise extent of his belief respecting contagion, I thought it right previously to inform him, that, since my release from quarantine, I had been already received at the British Ambassador's, and other houses, and to wait his permission to visit him. He immediately sent to invite me to dinner. This learned and respectable gentleman had, at an early period of life, been bred to medicine; had been educated at Edinburgh; and intimate with Sir John Pringle, Drs. Cullen, Munro, and other eminent physicians, their cotemporaries. During dinner, I enquired what foundation there might be for the story so current in the Levant, respecting the two hundred Russian soldiers, stated, by Sonini, to have died of the plague, for which they were said to have been inoculated by the surgeon of the regiment, to which they belonged, and who was said to have perished with them. A Russian gentleman, at table, assured me, that, so far from there being any truth in the story, the regimental surgeon alluded to, is now alive and well: and he mentioned his name and place of residence, which I omitted to note, and have now

forgotten. To me, who know that it is impossible the plague should be produced by inoculation, it could not have required any contradiction, to render this fable incredible. But, for the satisfaction of others, I am glad to have it in my power to announce, from so respectable a quarter, the formal disavowal of a tale, which, although it could only have been imposed upon Sonini, by some of the plague dealers of the Levant, or their dupes, is calculated to operate powerfully upon general credulity.

Mentioning, in the course of conversation, at Mr. D'Italinsky's, that, among other articles, I had found the tincture of litta an efficacious remedy in some cases of plague, a gentleman present informed me, that a Russian practitioner, who had made some experiments on the subject, had found the powder, given internally, a good remedy.

Mr. D'Italinsky, having shewn a warm interest in my investigation, I presented him, for perusal, with a copy of the Report which I had addressed to Sir Robert Liston, requesting, as I had not another copy, that he would have the goodness to transmit it to me to Pera, after he should have finished. In a few days, I received the Report, accompanied by the following letter, together with letters of recommendation to Count Stakelberg, the Russian Minister, at the Court of

Vienna, and to Count Lieven, the Russian Minister, at the Court of London.

Buyukderé, 7⁷/₉ Octobre, 1815.

Monsieur,

Vous trouverez ci-joint le manuscrit, dont vous avez eu la bonté de me permettre de prendre lecture : je vous en remercie infiniment : tout y est extrêmement intéressant, et vos observations sur la nature de la maladie, et les moyens tant pharmaceutiques que dietetiques, dont vous vous êtes servi contre elle, et qui mettent en évidence, que la peste n'est pas plus invincible que ne l'a été Bonaparté¹, malgré l'opinion, où bien du monde a été à cet égard. Quel service inappréciable vous avez ainsi rendu à l'humanité ! Curtius s'est dévoué pour le salut de ses concitoyens : votre plus heureux dévouement regard le genre humain. Penetré de sentimens dûs à votre noble heroïsme, j'ai l'honneur d'être, Monsieur,

Votre tres humble et tres

Obeissant Serviteur,

A. D'ITALINSKY.

A Mr. Maclean, Pera.

From the house of the Russian Ambassador, having taken the precaution previously to an-

¹ The overthrow of this extraordinary man, and the immediate consequences of that event, were fresh before the world, when this letter was written.

nounce my intention, I went, in the evening, to Baron Sturmer's, the Austrian Internuncio, in order to get the passport and letters, for which Mr. Frere had the kindness to apply to him, for the purpose of facilitating my journey through the Austrian dominions, and procuring, if practicable, some remission of the usual period of quarantine at the borders. Upon entering the court, and ascending a flight of stairs, I observed considerable confusion amongst a party, principally of ladies, in an adjoining apartment. By this time I had advanced so far in front of the entrance into the saloon, that their retreat, that way, was cut off. But suddenly divining the probable cause of the consternation, which appeared to prevail, I instantly receded some steps toward the landing place, giving an opportunity to the ladies to re-assure themselves, and time to the Secretary to measure the presumed infectious distance. This gentleman, advancing from the company, approached me, and we retired into a distant chamber, where, after some conversation, he gave me the passport, in question, promising to forward to me at Pera, letters for General Kienmayer, Governor-General of Transylvania, and for Mr. Fleischackle, the Austrian Agent, or Consul, at Bucharest. These letters were afterwards of considerable service to me. The worthy Internuncio, I regret to hear,

has since had the affliction to lose one of his family by the plague.

On the eve of departure, I felt desirous to manifest, by some trifling offer of good will, my sense of the uncommon civility and attention, which I had uniformly experienced from Mr. Pisani, and of the chearful as well as able manner in which he had executed the troublesome part of the functions, that were assigned to him, in the negociations with the Porte and the Greeks, respecting the commencement and proposed resumption of my investigation. Having, with this view, begged his acceptance of a barometer, which I had brought from England, accompanied with such expressions of thanks, as I considered due¹, I received from him, in reply, the following polite communication :

¹ Of this letter I did not keep a copy. It will also be observed that I have omitted the insertion in the journal, of the notes, which I had occasion to write, when at the hospital, to various correspondents, even when I have thought it necessary to insert the answers to them. For this omission there is no other reason than that, having been written in haste, whilst the messengers, who were to convey them, were kept waiting outside of the gates, they were not deemed of sufficient consequence to occasion the detention of those messengers, in order that copies of them might be taken. I am not aware, that any injury has arisen to the investigation, from this omission ; or, that, if copies of all of them had been kept, the insertion of any of them would have

Pera, 20th October, 1815.

My dear Sir,

Your very kind letter of yesterday, has afforded me the highest pleasure, at the same time it filled me with a greater degree of mortification than I can well express.

Your personal worth, and the generous object of your journey to this country alone, not to allude to the respectable recommendations you brought, give you so strong a title to the attentions of every one, that I declare I have done nothing equal to your merit, and nothing equal to what I really wished.

I, therefore, beg you will take the will for the deed; and as you have thought proper to send me so rare a thing in this country, as the barometer you brought out from England, I accept it, as a token of your friendship and esteem; requesting your acceptance of the assurances of the most perfect truth and respect, with which I have the honour to be,

My dear Sir,

Your devoted, and very faithful

Humble Servant,

B. PISANI.

been found necessary. But, in the progress of an investigation, at once so delicate, important, and dangerous, I admit that no circumspection, could have been superfluous.

A few days previous to my departure from Pera, received the following communication, from Dr. Küttel, a physician of Pest, in Hungary, which, as it relates so immediately to the subject of my researches, I do not think myself at liberty to suppress.

*Spectabilis et clarissime Domine, Collega
Charissime !*

Cùm hisce diebus in Ephemeridibus publicis legerim, te tam eximio, quam nostris his temporibus insolito, artem saluberriman in ægrorum Peste Ægyptiacâ laborantium curatione promovendâ zelo flagrare; ut nosocomio, quod Græcorum pietas huic scopo consecraverat, præfici volueris; et ipse olim terribilem hunc morbum, in confinio patriæ ultra annum tractandi occasionem habuerim; præsentibus eò minus molestus tibi futurus esse metui, quòd eadem studia animos jungere ac sociare semper observatum sit; totius adeoque orbis medici semet uti amicos et commilitones, considerare et gerere meritò debeant. Hac consideratione fretus, otium tuum paulisper interpellandi animum sumsi, non ut in re Europæis medicis peregrinâ, prolaxis ratiocinationibus patientiam tuam fatigem, sed ut te perhumaniter rogem: experimentum quod bono ut videbatur quondam successu cæperam, pluribus occasionibus tibi non defuturis, si videtur continuare velis;

quo refers ita felix existas, ut palmam, quam mihi voto nonnisi concipere occasio præceps permiserat, bene meritam referas, gloriamque inventæ contra Pestem certæ Prophylaxeos æternam consequaris: misertus videlicet tristissimi status, quem rudis quidem populus, uti plurima alia sua mala bona fortuna non sentit, sed in quo omnis, post contagium pestilentielle contractum pertres subsequentes hebdomades versatur; quarum quovis momento, eruptionem morbi hujus protheiformis truculentissimi formidandam habet, aliqua ardefacta systematis nervis alteratione illam penitus præpedire, aut si hoc non liceret, exardescentem nihilosecius leniorem reddendi ac mitigandi artem adinvenire desiderans, moderatum mercurii usum huicæ indicationi maxime respondurum multis rationibus ductus credideram. Atque sancte tibi affirmare audeo me complures casus, tam interne debita dosi prouti nempe videbatur prudenter auctæ et imminutæ, quum etiam externe inunctione circumspecte provocatæ ac fotæ febriculæ hujus mercurialis vidisse, qui spem concæptam plurimum confirmaverint. Cum tamen si qua alia uti novisti in arte, certe in nostra, tam falli quam fallere perquam facile esse soleat; te præsentibus iterum iterumque rogo et moneo, ut meliorem medicum eum recogites, qui morbum prevertat, quam qui curet; atque pluribus experimentis sedule institutis, num res sic se habeat

necne extra dubitationem omnem ponere satages. Quod gradum febriculæ dictæ per totum stadium contagii h. c. tres septimanas penitè dixeram sustentandæ concernit, monere debeo : me eandem nunquam ultra auxisse ; quum ut valetudinis statum contaminatorum quam minime licuit alteraverim ; et cuncta vitæ munia, prouti sanissimi obire valuerint. Cæterum usui mercurii, apud sanos nil præmisi, secus ubi videbatur leve purgans ; aut vomitorium, interdum potum Thee ex herbis aromaticis leviter amariusculis adjunxi. Quod ppta attinet, interne, calomel aut merc. ciner. Sanderi, externe unguentum neap. adhibui. Forma vero pilularum, quas ipse omni sedulitate confeceram, oportunissima videbatur. Tandem monere debeo me prophylaxim talem non ob unam aliamque probabilem contaminationem prægressam suscepisse ; quippe qui ipse quoque pluribus vicibus, et ægros et res ab iis usitatas impune contrectaverim ; sed apud tales, ubi consortium intimius pluribus diebus ac septimanis intercesserat. Qui nempe suis amicis, cognatis, uxoribus, maritis, prolibus, parentibus, ex peste ægrè decumbentibus diu noctuque omnia ministeria vivis ac mortuis sine omni cautela fidelissima præstiterunt, cum conductitios infirmarios nullos habuerim, neque adhibere voluerim. Quiscunque tandem futurus successus sit, uterque nostrum quo circumstantiis in magnis voluisse sat

eum credemus. De morbo nil dico, cum inter decem (x) vix duos similem habere solere quotidie videas. Queis cum constantem valetudinem etsalutarium tuorum conatuum in solatium generis humani optimum successum cordicibus adprecor memet amico tuo favori commendans maneo.

Sincerus tuus Amicus,

JOH. KÜTTEL, M.D.

D Pesslenii, Die 29 Sept. 1815.

It cannot fail to be remarked, that the practice of the internal and external administration of mercury, recommended in this letter, is only a repetition, but imperfectly understood, of that which I have myself employed, in fevers, and some other acute diseases, since 1788, and of which the outlines were promulgated in India, in 1796. It is not, indeed, to be imagined that Dr. Küttel could have been, in the smallest degree, aware, that he was only transmitting to me a faint copy of my own ideas. But still less is it to be imagined, that, after these ideas had been in circulation for twenty years, in Europe, Asia, and America, and my dissertation upon epidemic diseases even translated into that gentleman's mother tongue, and published at Leipsic, and at

Cobourg, in Saxony, in 1805, they could have been, in him, original. The plagiarism, however, I do not suppose to have been, in this case, wilful; and the information was, doubtless, conveyed with the best possible intentions: and, as emanating from some personal experience of the efficiency of the treatment recommended, if addressed to a person, who happened not to have had any practical knowledge of a similar kind, might have proved of much use.

Dr. Küttel, I trust, will now admit that I have discovered the true prophylactic of epidemic diseases, of which he speaks as a desideratum; and that I stand exclusively entitled to whatever credit may be thought due to such a discovery. The error is not peculiar to this gentleman, that the Pest Depôts have been instituted by the charity, or piety of the Greeks; whilst they are only consequences of the fears of the many, and the cupidity of the few.

BOOK VIII.

ATTEMPTS, MADE BY OTHERS, TO INVESTIGATE THE PLAGUE.

For man loves knowledge, and the beams of truth
More welcome touch his understanding's eye
Than all the blandishments of sound his ear,
Than all of taste his tongue.

PLEAS. OF IMAGIN.

CHAP. XXXIII.

“Conclusions in natural philosophy are proved by induction of experiments, things moral by moral arguments, and matters of fact by credible testimony.” TILLOTSON.

Of the casual attempts, which have been made, by other persons, to investigate epidemic diseases—by Howard—in Egypt—in Gibraltar and Spain—universal employment of inadequate evidence—why the researches of Drs. Whyte, Valli, and Mr. Von Rosenfeldt, deserve to be more particularly noticed.

TOWARD a complete view of the obstacles, which have hitherto opposed the investigation of epidemic diseases; of the progress, which has been made, in elucidating this interesting subject, by

the results of my experiments in the Levant ; and of the facilities, which have been thereby established, for accomplishing what may yet remain to be done, in order to render the enquiry complete ; I think it right to annex, to my own proceedings, some account of the attempts, which have been made, by other persons, to investigate the plague of the Levant.

In doing so, I shall have an opportunity of correcting some gross misrepresentations, which have gone abroad, both in discourse, and in the public journals, respecting the nature of my own experiments ; as well as of distinctly shewing the cause of the failure of my intrepid fellow-labourers, by which unhappily the pre-existing delusions have been farther prolonged. But the principal object, which I aspire to attain, by the present disquisition, is to convey to future investigators (if it should be possible, that the results, which I have been so fortunate as to have already obtained, should not have the immediate effect of producing a general conviction of the necessity of abolishing the system of police, established in consequence of the prevailing errors) a knowledge of the nature of the dangers, which they will have to avoid, and the obstacles, which they will have to encounter, in investigating the plague of the Levant. Such knowledge may happily be the means of saving the lives, which might other-

wise continue to be uselessly sacrificed, of men, who, urged by a laudable, but too ardent zeal for discovery, might be led to encounter risks, of the extent of which they cannot, but from the information of those who have preceded them in the same path, be rendered sufficiently aware; and which are, in their nature, almost insurmountable.

The nature of these dangers, and of these obstacles, being duly explained, it will not appear surprising, that scarcely any of the persons, who have attempted to investigate the plague of the Levant, should have escaped with life; or that their attempts should not have been followed by any useful results. As far as I have been able to learn, every physician, who has hitherto ventured upon the enquiry, in a regular way, in any of the pest houses of that country, with the single exception of Dr. Valli, has been the victim of his enterprise¹; and, from this circumstance, conclusions have been drawn anew, in favour of contagion; as if physicians were not equally liable with other men, to be affected by a pestilential atmosphere. Such is the facility of confounding, or rather

¹ At Odessa, during the last great plague, in 1812, in which the Duke de Richelieu, who was then governor, saw that every man performed his duty, four physicians, as that nobleman informed me, died; being, I believe, all the physicians in the place.

the difficulty of not confounding, the effects of atmospherical influence, with those of contagion, where the existence of the latter is previously taken for granted, that diseases, which had been regarded, by the medical faculty, as purely epidemic, until some of their own body had been seized, were afterwards, apparently for that reason only, considered by them as contagious¹.

In this narrative, it will be proper to distinguish those parts, which are only traditional, and consequently, as to scientific results, altogether to be disregarded, from those, which, although not certain and conclusive, may, however, be entitled to some degree of consideration. Amongst the former, is the story of the Russian regimental surgeon, so current in the Levant, of which I have, at p. 226, given the formal contradiction².

At the hospital of the Seven Towers, I was informed, that another Russian practitioner had, some years ago, in that place, killed himself, by laying upon ice, whilst he had the plague; as if the plague were not sufficient to kill, without the ice. It is also related, that, of two German phy-

¹ This happened during the plague of Marseilles in 1720.

² Sonini's Travels in Greece and Turkey, p. 497. We now know that this story cannot possibly be true, because it stands demonstrated that the plague is not contagious, and because, by inoculation, a non-contagious disease cannot be propagated.

sicians, who were sent, by the Russian government, to investigate the plague at Smyrna, one died in the hospital (the Greek Hospital I presume) of that malady, in three days, and the other ran away, and was not afterwards heard of. It may, indeed, be believed, because it is not incredible, that such persons have existed, and that they entered the Pest Hospitals, and there perished; but this is all that can be known, to a certainty, of the history of those that die. How, indeed, should the tales, related of the dead, be entitled to any credit, when we find the grossest and most preposterous falsehoods, systematically circulated, respecting the living? It was, for instance, deliberately and gravely asserted of myself, that I inoculated myself with pestiferous matter; that I died on such a day, and such an hour; that my interpreter had the plague; and that the servant, who did *not* accompany me to the hospital, had also the malady. Nay, at the very time that I was visited, in quarantine quarters, by Sir Robert and Lady Liston, and all the British subjects resident at Constantinople, and when my health was confirmed, and I was daily to be seen walking abroad, it still continued to be asserted that I was dead; but that the ambassador, and the rest of my countrymen, were concealing my death.

The manner of it was variously related, some reporting that I had died of the plague, others of

poison administered by the Greek priest, and others of strangulation. There were persons, who went so far as to assert, that the disease, which killed me, was but a very slight one ; and others, that I had no disease at all.

———— Rumour is a pipe,
Blown by surmises, jealousies, conjectures ;
And of so easy and so plain a stop,
That the blunt monster, with uncounted heads,
The still discordant wavering multitude,
Can play upon it. HENRY IV.

The reader will excuse my dwelling upon these personal circumstances, more than might otherwise be proper, since it is necessary to the full elucidation of my subject. The assiduity with which these reports continued to be circulated, evinced that the event was considered of no small importance by the plague-dealers of the Levant. The report of my death was, no doubt, first promulgated, and afterwards maintained, by those who best knew the falsehood of it, in the probable anticipation of what they, and the plague, might eventually be able to accomplish. Fortune had always been hitherto in their favour ; and they no doubt reckoned that the blind goddess would again prove propitious to their wishes. But they were, this time, destined to be mortified and disappointed. If their hopes and anticipations

had been fulfilled, the results of my researches would have been lost, or destroyed; and their falsehoods would, for a period which it is impossible to determine, still continue to pass current for truths upon the world¹. Even those who had no direct interest in the failure of the investigation, might have particular motives for not wishing it success. To be instructed by a stranger was to inflict an incurable wound on self-love. Bigots, who might think it sinful to disbelieve a doctrine issuing from the Vatican, would consider death a just punishment for heresy. That which we either wish, or expect, we readily believe. But here, both experience and reason justified credulity. Indeed the most absurd expectation would have been, that any one could escape alive, from a pest-house in the Levant, professing an investigation, upon the avowed principle of non-contagion, and with the view to destroy the existing system. The manner, in which this piece of good fortune happened to me, has been already related: and how much and sincerely the event was deprecated, may be inferred from the deter-

¹ It was the publication of the first volume of this work, which called forth Dr. Whyte's papers from obscurity; where, had the results of my researches been lost, they would necessarily have remained, until some other person had made similar discoveries, which might have led to their just appreciation.

mination not to give me the opportunity, by means of an establishment, placed at my entire command, of shewing a second time, how little the plague alone is to be dreaded, under a proper system.

In taking a view of the researches, which have been undertaken by other persons, concerning the plague of the Levant, it will be proper to premise, that, previous to 1796, the belief almost universally entertained of the contagious nature of epidemic diseases, (for the few, who happened to entertain, with respect to particular epidemics, an opposite opinion, do not appear to have done so upon any sufficient grounds of conviction) necessarily precluded all attempt at experiment, in respect to the cure, with any probable prospect of success. When the physician could not place himself in contact with the pestiferous patient, without the apprehension of being himself infected, and of communicating the infection to others, he could only prescribe at random, and without any certainty that his prescriptions would be applied, or rather with the certainty that they would not be applied; for the "*servitores et ministri*" cannot be supposed not to have entertained similar apprehensions, and at least to an equal degree, with the "*medici honorati et nobiles.*" But, even if it could be presumed, that, in a fever like the plague, the physician,

without feeling the pulse of his patient, could be able to form proper indications of cure ; and that the attendants upon the sick, notwithstanding their fears, would implicitly enforce his directions ; it must be recollected, that any plan of treatment then known, consisting for the most part of bleeding and purging, were, in such diseases, either worse than useless, or wholly inefficient.

Previous to the commencement of this century, the benevolent Howard appears to have been the only person, who has deserved to be considered an investigator of the plague. But the prevailing opinions, respecting the cause of epidemic diseases, not having been generally called in question, in his time, the views of that illustrious friend of humanity did not, as we have seen, extend beyond the improvement of Lazarettos, upon the principles on which they had been originally established. His belief in contagion was, indeed, so implicit, that he even considered it criminal in others to doubt ; and it was principally to this unhappy delusion, as has been shewn, that he prematurely fell a lamented victim.

At the close of the last, and the commencement of the present century, an event happened, which seemed eminently calculated to produce an acquisition of knowledge upon this interesting subject. The invasion of Egypt, first by a French, and afterwards by a British army, promised to open

a new and extensive field for observation. The facilities of research, thus afforded to the medical officers of two great and rival nations, in a country reputed the very cradle of pestilence, were such as had never before occurred : and these facilities were farther enhanced, by the exemption enjoyed in armies, from those considerations, which usually restrain the private practitioner, within the trammels of an accredited set of forms ; or rather by the entire freedom of opinion, which is commonly enjoyed in military medicine, as well as the exact conformity with prescription, which is ensured by discipline, and habits of subordination.

But, on the other hand, it must be admitted, that the exigencies of warfare seldom allow of the leisure, or leave access to the aids, which are necessary to successful investigation. And the belief in contagion, which still continued to pervade armies, as well as civil life, would infallibly paralyse the efforts of those, who might be the most disposed, as well as the best qualified, for investigation.

Hence, it ought not to surprise us, that these expeditions should not have produced any results of importance, respecting either the prevention, or cure, of pestilential maladies. Contagion, as their cause, continued to be, as usual, implicitly taken for granted ; and certainly nothing has been laid down, respecting the cure, which deserves

to be retained amongst the means of any permanent plan of treatment.

We know, indeed, that some of the medical officers, as the physician in chief of the French armies, did conceive it expedient, with a view to tranquillize the minds of the soldiers, to hold forth the idea, that plague does not depend upon contagion, or that the diseases, which afflicted the troops in Egypt, were not of the nature of pestilence¹. But, it does not appear, as far as

¹ Baron Desgenettes, Physician in chief to the French army, for having pursued this policy, and refusing to give the name of plague to the disease, with which the soldiers of that army were afflicted, in Egypt, assigns this motive: "Je crus devoir dans ce circonstance traiter l'armée entier comme un malade, qu'il est presque toujours inutile, et souvent fort dangereux d'eclairer sur sa maladie, quand elle est tres critique." Being a decided believer in contagion, it would have been extremely inconsistent in Baron Desgenettes to have seriously inoculated himself with pestiferous matter, since he must have expected the disease to have been thereby propagated. But to have feigned inoculation, and to have represented the experiment as having failed, would have been perfectly conformable both with his belief, and his view of tranquilizing the minds of the soldiers. In the manner of relating this experiment there is something that appears still to require explanation. Of the result he says, (p. 89.) "*Elle n'infirmé point la transmission de la contagion, démontrée par mille exemples; elle fait voir seulement que les conditions nécessaires pour qu'elle ait lieu, ne sont pas bien déterminées.*" It is, however, in my opinion,

I have been able to learn, that a disbelief in contagion, or even a doubt respecting it, had been entertained, upon the true grounds, either by him, or by any of the other persons employed upon either of these expeditions.

In an army almost exclusively composed of Catholics, like that of France, however their commanders, and medical officers, might, from views of political expediency, have been disposed to represent the diseases to which they were more particularly exposed, as depending upon other causes than contagion, it was not to have been expected, that any of them, upon such grounds as were then known, could have permitted themselves, *bonâ fide*, and at once, to deviate so widely from the creed of their ancestors, for so many generations, as to think it necessary even to enquire, whether the great mother pestilence, of Egypt, does, or does not, depend upon a specific contagion.

For reasons connected with medical education, as traced in the first volume of this work, the prejudices, respecting contagion, have been

sufficiently determined, that, under no conditions, can such an event happen : and, instead of being “ demonstrated by a thousand examples,” I aver, that it cannot be proved in a manner that ought to satisfy the scientific enquirer, that plague, or any epidemic disease, has ever, in a single instance, been propagated by contagion.

scarcely, if at all, less inveterate among Protestants, than among Catholics: and they were, perhaps, equally strong in the British, as in the French army. I cannot, however, but feel some surprise, that, in the former, especially that part of it which came direct from India to Egypt; no attempt should have been made to ascertain the validity of the principles, in respect both to the cause, and to the cure of epidemic diseases, which I had promulgated in that country, several years before; and this omission appears to be the more unaccountable, since no other proposed mode of treatment had been found efficient; and, since, whatever might be thought, under the commonly received notions, of the danger of applying high degrees of remedial power, in ordinary degrees of disease; there could not, upon the vulgar maxim of the propriety, in desperate diseases, of employing desperate remedies, have been any rational ground of objection to their application, in such a malady as the plague.

Opportunities for investigating epidemic diseases, scarcely less favourable than those which occurred in Egypt, have been presented by the various pestilences, which have prevailed in Gibraltar, and in different parts of Spain, in the years that are past, of the present century; and many volumes have been written upon the subject, by the medical officers of the British,

French, and Spanish armies, in the Peninsula, who have been candidates for employment, or for fame. To enter into any detail of these writings would be both irksome and unprofitable. It may be observed of them generally, that, instead of profiting by the elucidations, which had previously existed, of the subject, they all, either by taking the papal doctrine of contagion for granted, in its fullest extent, and thus assuming the usual erroneous datum, inevitably fall into the usual erroneous consequences; or, by denying contagion, in particular pestilences, and by way of apology for the boldness of the innovation, instituting groundless distinctions between plague and other epidemic diseases, involved the subject, if possible, deeper in perplexity than before.

This failure has greatly depended upon the employment of evidence of an improper kind. Each department of knowledge has its peculiar, or appropriate species of evidence. Questions in science can no more be decided by authority, tradition, or testimony, than matters of fact, by reasoning, induction, or experiment. All the traditional, or testimonial evidence, the world ever saw, would be insufficient to prove a single proposition in the mathematical sciences, or natural philosophy. If, in these branches of knowledge, a conclusion had been attempted to be grounded upon the evidence of the collective

assertions, or oaths, of all the inhabitants of the universe ; and it had, upon that authority, been acted upon, and remained unquestioned, for thousands of years, it is certain that this would not render it, in the smallest degree, true ; but, that, on the contrary, it might be refuted, even in the course of a single day, by a few correct processes of reasoning and induction. Seeing, then, that medicine is a branch of natural philosophy, and that epidemic diseases are a principal branch of medicine ; it cannot surprise us to find, considering that this is the species of evidence, which the cultivators of medicine have almost universally and exclusively employed, that this interesting science should have hitherto, in general, made so insignificant a progress ; or that, in some respects, it should have even undergone a retrogradation.

This, as has been shewn, happened particularly in respect to epidemic diseases. The doctrine concerning their cause, which has prevailed for 270 years, was not only an immense retrogradation, but operated as a permanent bar to re-advancement, or farther improvement. At the periods of its promulgation, the principles of inductive philosophy had not yet dawned upon the world. The wretched fables, by which this doctrine was originally supported, having the then greatest human authority on their side, if

they durst have been opposed, were incapable of being refuted, by mere assumptions and assertions of an opposite kind. Where the decision of scientific questions is not made to depend upon the evidence proper to science, they can only incline to whatever side the balance of authority and numbers may chance to preponderate : and the chances, that authority and numbers, without examination, or contrary to views of interest, will judge rightly, are always infinitely small.

Under such a state of things, a doctrine like this, or one if possible more absurd, might continue permanently to prevail : or, at least, it would only vary from one form of absurdity to another, as power, influence, opinion, or fashion, might vary. With the prevalence of papal authority, the proposterous hypotheses of Fracastorius, and his successors, respecting the cause of epidemic diseases, have long pervaded Christendom, and have even survived the influence, which gave them birth. And if, in the decay of that authority, it should have appeared expedient to those, by whom it had been supplanted, either to have continued the same doctrines, or to have promulgated others of a diametrically opposite nature, they would, under an equal unity of power, and a similar disregard of fit evidence, alike prevail. In such case, the conclusions, although obtained by wrong means, i. e. an unfit

evidence, might happen to be correct ; but being built upon so unstable a foundation, they could not remain permanently established. The misfortune here is, that if, by the path of tradition and testimony, we should chance to gain sight of true inferences for a moment, (which however almost never happens,) we should, by continuing the same delusive path, soon infallibly lose them. Tradition and testimony are not necessarily attracted by the magnet of truth ; but will lead the enquirer, who unhappily trusts to them, in matters of science, to any point of the compass, to which the winds of power, influence, authority, interest, prejudices, or passions, may, for the time being, happen most strongly to blow. Thus, in respect to the cause of epidemic diseases, it may be said, that, hitherto, according to the logic of numbers, the testimonies of authors have given the decision entirely in favour of the papal doctrine of contagion ; but, that, recently, a similar evidence, on the other side, has, with respect to *some* epidemics, been accumulating, until it has attained a bulk extremely formidable ; that the numbers have, occasionally, been even nearly balanced ; and, that there is every prospect, that, in no long time, under the diminishing respect for papal infallibility, and the increase of a different species of influence, which may be that of a different modification of interest or fashion, the

number of voters against contagion will continue considerably to increase.

But, if, in the change of influence, and fashion, the balance of opinions should turn completely against contagion, although the conclusion would happen to be right, the result would still be nothing more than an opinion. The evidence would be no better than that which formerly existed in favour of contagion, the conclusion being erroneous. In such a case, unanimity itself would be incapable of securing permanent conviction.

It is evident, then, that in matters of science, no quantity of this species of evidence, can ever amount to a proof. If it were to be relied on, it might even happen, as was nearly the case with respect to the fevers of Gibraltar, in 1813, and 1814, that the opinions of medical men, in favour of each side of the question, might be equal; and in that case no decision could be formed, until some extraneous authority were called in to give the casting vote.

According to this mode of judging, respecting the cause of an epidemic, by a majority of opinions, the same process must be applied anew, not only to every distinct epidemic, but to every separate case of disease. And it cannot, until its termination, be ascertained, if then, which side has the greatest number of votes; i. e. when

the decision, if it could have ever been good for any thing, would come too late to be of any use : for it will not apply to any future fever ; since, as the nature of epidemics, according to the prevailing notions of medical men, who are to be the judges, is liable to various interpretations ; and is commonly, or almost always, a subject of dispute ; every fresh fever will require a fresh set of testimonials of character. And, in this state of uncertainty, supposing always that the evidence, as to the particular epidemic to which it had been last applied, were conclusive, (which, however, is presuming too much,) the experience of the preceding, cannot be rendered available, in the succeeding fevers, even of the same country. Thus, if a yellow fever, occurring this year, at New York, or Philadelphia, or a plague at Constantinople, or Smyrna, could, by this mode of reasoning, be shewn to depend, or not to depend, upon contagion ; the same conclusions could not be drawn, respecting a disease of similar phænomena, the following, or any other year, but by a repetition of the same processes. Thus, also, a fever at Gibraltar, according to its degrees of severity, and its most prominent symptoms, might, one year, be proved to be an endemic of the country ; another year, the yellow fever of the West Indies ; and, the third season, the plague of the Levant ; each time requiring

a new series of testimonials of character, which, if they were adequate, would come too late for the existing occasion, and be inapplicable to succeeding maladies.

But the mischief does not end here. A fever, with precisely similar symptoms, and of a similar degree, might be adjudged, at sea, to depend upon contagion, and, on shore, on some other agent; in different islands, and in the same island, at different times, to arise from different causes; in one country, to be endemic; in another to be indigenous; in a third, to arise from a foreign, and, in a fourth, from a domestic origin. In this manner, the logic of numbers might decree the same disease to be endemic, epidemic, indigenous, contagious, and of any name, origin, or country, at will.

Here, then, we have an interminable source of medical confusion and controversy. And, if this be a correct representation of the effect of deciding by testimony, tradition, and authority, it cannot be deemed surprising that all medical evidence should have been hitherto deemed uncertain. Yet such is the evidence, of which the results have, by writers on epidemic diseases, been dignified with the term *proofs*. These "*proofs*" have, in respect to the recent epidemics of Gibraltar and Spain, occupied numerous volumes, without producing, and, what is more, if they

should occupy as many volumes as would fill a world, they never could produce conviction. How appalling, if this mode of proof should be extended to every fever, that may hereafter occur, in different countries, or in the same country, and in different parts of the same country, at different times!

Thus, the reason is obvious, why the accumulated masses of testimonial evidence, on each side of this important question, which have been, at various periods, presented to the British Government, have not, and could not have had any other effect, than that of still farther embroiling an intricate subject. When, in 1805, in reference to the great objects of policy which it involves, the question of contagion, as the cause of epidemic diseases, was referred, by the Lords of his Majesty's Privy Council, to a Board of Health, the affirmative was, as usual, taken for granted; and the consideration was not, whether quarantine, and other institutions of plague police, should continue to exist, or be abolished; but what was the best mode of organizing them.

It was also upon evidence of the fallacious nature, which I have described, that the College of Physicians, of London, since they had no other, were obliged to form the *opinion*, that the fevers, which ravaged Cadiz, Malaga, &c. that year, were propagated by contagion.

In November, 1815, an appeal was again made to that body, respecting some propositions, then newly invented, in the Iberian Peninsula, to support the tottering cause of contagion, exceeding, in extravagancy, any thing that had, of late years, appeared upon the subject: on which occasion the College gave a guarded and a very judicious report.

Should the principles promulgated in this work, as is probable, be submitted to their consideration, they will, I trust, be found to be the result of evidence of a very different kind. It has appeared to me, that, it is only by taking a general view of the laws of nature, as they respect the whole class of epidemic diseases; and thus deducing a series of broad inferences, from numerous and acknowledged facts, that we can arrive at any certain, or satisfactory conclusions. To assign different, or opposite laws, to different epidemics, is, I think, about as wise, as to assign a different, or opposite nature, to the different individuals of the human species. The reasoning, which applies to any one, must apply to all; and the reasoning, which applies to all, must apply to each individual, of this class of maladies. Whatever evidence is capable of proving that any one epidemic, depends, or does not depend, upon contagion, must be equally capable of proving the same thing, in respect to

every other. It cannot possibly happen, that some should be proved to depend, and others not to depend, upon that source. To maintain such a position, is to advocate both sides of the question: and is even more grossly inconsistent, than the conduct of those, who maintain, that all epidemic diseases depend upon the same cause, although the cause, which they assign, should happen to be erroneous. Those who assign the same cause for all, if it should not be the true one, will fall but into one error; whereas, those who assign a different cause for each, if they should not be correct in any, as has hitherto invariably happened, will fall into as many errors, as there are different epidemics.

This much of the reasons, why the desultory efforts, which have been casually made, to investigate epidemic diseases, have failed. There are, however, three persons, whose proceedings, as they entered upon a more regular mode of enquiry, and as instruction may be derived from their very failure, deserve to be more particularly noticed. These are, Dr. Douglas Whyte, a Briton; Dr. Eusebius Valli, an Italian; and Mr. Von Rosenfeldt, a German. The researches of these gentlemen, according to the best authenticated accounts, which have come to my hands, shall be detailed in the following chapters.

CHAP. XXXIV.

EXPERIMENTS, AND DEATH, OF DR. DOUGLAS WHYTE.

IN October, 1817, several months after the publication of the first volume of this work, some papers, which had been transmitted, in 1801, from Aboukir Bay, by Dr. Whyte, to the Levant Company, were put into my hands, by Mr. Liddell, their secretary'. From these papers, it appears, that he was at Constantinople, in the months of September and October, 1800; but did not enter any of the Pest Hospitals there, probably because they contained no plague cases, at the time; or, perhaps, because he was aware, which I perceive to have been the case, of their extreme turpitude.

From thence he passed over to Egypt; and we find him making experiments in Aboukir Bay in May, June, and July, 1801. A certificate,

' The amenity, attentions, and friendship unremittingly displayed by this gentleman, to myself, as well as to my family, in my absence, during this investigation, it would be callous not to feel, and ungrateful not to acknowledge.

dated the 1st of August, and signed by Captain Gunter, the principal agent, and two masters of transports, Williams, and Porritt, declares his having inoculated himself in the arm, and rubbed on his thigh, some matter taken from the pestilential abscess, of William Denning, seaman of the Betsey transport; that they were of opinion the disease with which W. Denning was afflicted was exactly what the people of the country consider as the plague; that he was carefully attended, and frequently touched and handled during his illness, by the master, servants, and seamen of the Betsey, without any of them, or of the soldiers' wives and children, with which the transport was crowded, falling sick therefrom, although no precautions were employed; and that Dr. Whyte neither caught the disease himself, nor was the means of communicating it to any of the crews of the other transports, then laying in the bay, of which he visited the greatest part in the course of his duty.

Amongst these papers, I find another certificate, signed by Captain Gunter, on board the Ellen transport, dated, in Aboukir Bay, 7th of August, 1801; in which it is affirmed, "that Dr. Douglas Whyte has treated on board of different transports in this bay during the last three months upwards of seven hundred cases of fever, flux, and liver inflammation, about twenty of

which were accompanied with those symptoms, viz. buboes and blotches, said to characterise plague, &c.”

“ I likewise certify,” adds Captain Gunter, “ that the said Dr. Whyte, by inoculating himself, and by various arguments, has demonstrated to my satisfaction, and to the satisfaction of most of the masters of transports, that the symptoms said to characterise plague are only those of a neglected, or mismanaged fever, and that they are neither infectious, nor contagious, nor incurable.”

From the papers before me, it does not appear, that Dr. Whyte had any fixed grounds, such as are assigned in this work, for the conclusion that the plague (for he does not seem to have extended his views to epidemic diseases in general) does not depend upon contagion; or that he had acquired any correct notions of the principles of cure. His principal idea of the treatment was to bleed “ to the extinction of pain;” i. e. in other words, *usque ad deliquium animi*, or to the extinction of sensation.

A variety of statements, none of them entitled to the smallest credit, because it can be shewn that it is impossible any of them should be true, have been sent abroad, respecting the experiments, and the fate, of this intrepid friend of humanity. It cannot be surprising that the

rumours propagated, on such occasions, by the plague dealers of the Levant, who are the natural enemies of investigation, should consist only of tales invented for the purpose, when we find the studied relations of authors, deliberately communicated to the world, and to be transmitted, if they should possess materials of durable existence, to posterity, so much at variance, in these matters, with truth and accuracy.

Respecting the death of Dr. Whyte, and the cause of his disease, a recent writer, has the following observations: " Whether Dr. Whyte, who entered the Pest House of the Indian army at El Hammed, early in January, 1802, was misled by this experiment (that of inoculation by Baron Desgenettes) I know not; but from a persuasion that the plague was not contagious, he immediately rubbed some pus, taken from a pestilential bubo, upon the inside of his left thigh, and the next morning inoculated himself in the wrist, with matter running from another bubo. Four days, however, had scarcely elapsed from his entering the Pest House, before he was seized with shiverings, followed by febrile heat, &c which he flattered himself would prove to be an intermittent. But he died of the plague before the end of the third day; and thus, unfortunately, added another to the proofs, alas! too many, of the contagious nature of this terrible

disease'." Such are the "*proofs*," which are, alas ! too often regarded as conclusive in medicine.

Not only had Dr. Whyte, as I have shewn, made the experiment of inoculation upon himself, in Aboukir Bay, before any account of the experiments of Baron Desgenettes were published ; but it is notorious, that he left England, in 1800, with the belief, that the plague does not depend upon contagion, and with the determination to subject himself to the experiment of inoculation, with a view to ascertain the validity of that belief. The surmise, therefore, that he might have been misled by the experiment of Baron Desgenettes, is obviously unfounded. The inference, also, that he was affected with the disease, in consequence of the inoculation in the Pest House of the Indian army, at El Hammed, would have been wholly gratuitous, even if it did not stand demonstrated, that it is impossible this disease should ever be communicated by contagion ; for, as he had twice previously inoculated himself with impunity, even if there were proof of the last alledged inoculation, having been actually performed, it would, in fair argument, be two to one against the malady having been at all produced by the inoculation, and in favour of

' An Essay on the Disease called Yellow Fever, &c. By E. N. Bancroft, M. D. &c. p. 584, et seq.

its occurrence from the operation of the real causes, at a period incidentally coinciding with that of the inoculation. But there are no proofs beyond bare assertions, and those perhaps of persons who had not even seen him when ill, of any of the circumstances relating to the death of Dr. Whyte.

Supposing the disease, of which Dr. Whyte died, to have been actually the plague, it is probable, as his belief did not amount to conviction, founded upon demonstrable conclusions, that, upon finding himself seized with the malady, the old familiar belief in contagion, like that of ghosts, as explained in the preceding volume, might have recurred to the enfeebled mind, and overwhelmed him ;

————— We are not ourselves

When nature, being oppressed, commands the mind
To suffer with the body.

KING LEAR.

Or, it is, at any rate, clear, both from his mode of prescribing for other patients, as related in his papers, and from his own fate, that he did not understand sufficient of the principles of cure, to remove a plague of ordinary intensity. And, from thence it is manifest, that, however ardent his zeal, and benevolent his intentions, Dr. Whyte wanted some of the primary essen-

tials, which I shall afterwards shew to be indispensable to success, in an investigation of this nature.

Where the plague is believed to be contagious, i. e. every where amongst Christians, nothing can, of a certainty, be known of the proceedings of those who are seized with it, if they die; or of the conduct of their attendants towards them. Their papers, if they should have written any, are either, from fear of contagion, or the dread of an evil, still more to be deprecated by the inmates of Pest Houses, the developement of truth, neglected, or destroyed. Hence, the circumstances attending the demise of persons so situated, are invariably misrepresented, according to the interests, prejudices, and passions of the narrators. A farther elucidation of these remarks shall be given, when I come to detail the proceedings of Mr. Von Rosenfeldt, which have been made the vehicle of much injurious delusion.

But to return to Dr. Whyte: respecting the results of his researches, it may be observed, that, although the number of patients, which were under his care, in Aboukir Bay, were considerable, and the proportions of recoveries appear, at first sight, to be uncommonly great; it ought not to be overlooked, that, in regularly organized communities, as in ships, and in regiments, where the sick are sent to the hospital,

or infirmary, the moment of their being seized, and therefore nineteen-twentieths of the cases are necessarily extremely slight, the deaths, under such circumstances, whatever be the medical treatment, cannot be numerous: and the mortality cannot be received as a criterion of the quality of the practice¹. Dr. Whyte's appears to have been of the ordinary kind, as is evinced both by his description of its nature, in the case of other patients, and the results of its application in the malady, which unfortunately terminated his own existence. In the course of his researches, he does not appear to have been exposed to any other danger, than that arising from the disease.

With respect to his experiment of inoculation, I have, in another place, shewn the fallaciousness, in general, of such a mode of proceeding; and the erroneous inferences, which have been deduced by others, from those which were stated to have been made by Dr. Whyte, in Egypt, are all in corroboration of my conclusions. But, it must also be admitted, that, in as far as such negative evidence can be received in proof of scientific deductions, Dr. Whyte's experiments,

¹ Of all the patients attended, by Dr. Whyte, in Aboukir Bay, five only are said, by Captain Gunter, to have died. Of his practice, I shall endeavour to form a more precise appreciation, in treating of the cure of epidemic diseases.

in the way of inoculation, as related by himself, have been not wholly unsatisfactory¹.

In this line of research, no man has deserved better of society, than Dr. Whyte. His zeal, and his courage, were great and conspicuous. I have been informed by Admiral Bissett, that, in a ship, which he commanded, coming from the West Indies, in which there was much sickness, Dr. Whyte, who acted as a volunteer, displayed the same ardor of research, the same unwearied diligence, and the same humanity to the sick under his care, by which he was afterwards distinguished in Egypt. It does not appear that this worthy man was ever advanced, in any degree, according to the measure of his merit; nor do I believe, that even a stone is placed to mark the spot in which so much ardent philanthropy is deposited. At least, we ought to be able to exclaim—

See nations, slowly wise, and meanly just,
To buried merit raise the tardy bust.

¹ I make this distinction, because, whilst those, which were related by himself, as having been made in Aboukir Bay, may be admitted to be authentic, I see no satisfactory evidence respecting those, which, since his death, have been said to have been made by him at El Hammed.

CHAP. XXXV.

EXPERIMENTS, AND DEATH, OF DR. EUSEBIUS VALLI.

Not having been able to meet with a copy of Dr. Valli's Journal of the Plague at Constantinople¹, in that city, Vienna, Paris, or London; and deeming all the traditions which I heard related, in the Levant, respecting his experiments, as entitled to little credit, I found myself but scantily supplied with materials, for writing an account, as I was desirous of doing, of the proceedings, and death of that intrepid investigator; when I had the gratification, only a few days ago, of being relieved from my embarrassment, by the receipt of a very satisfactory account of these proceedings, published in the New York Medical Repository for January, 1817, and kindly transmitted to me, by Dr. Felix Pascalis, one of the editors of that independent Journal. From this source, and some information, derived from other quarters, I have been enabled to extract materials for a statement, which, there is reason to believe, will be found to be accurate.

¹ The title of Dr. Valli's work is, " Sulla Peste di Constantinopoli del 1803, Giornale del Dottore Eusebio Valli, &c. 1805."

Dr. Valli was a citizen of Florence, and first physician, and clinical professor, in the Civic Hospital at Mantua. He went to Constantinople, in the year 1803, without any expectation of being able to apply an efficient method of cure to the plague, or any opinions other than those commonly prevailing, respecting its cause and prevention. Those, who have perused with due attention the first volume of these results, will not expect to find, that researches, conducted upon such grounds, should, unless incidentally, lead to any useful discovery. His principal idea appears to have been, that the matter of plague might be rendered mild and safe, for the purpose of inoculation, by being mixed with the matter of small-pox. He accordingly inoculated himself with these two fluids, in the month of July, in the French Hospital at Pera. This inoculation, it seems, was followed by an agitation, and suffering, which continued for seven days, as might have been the case from the introduction of a hundred thousand different kinds, and combinations, of equally acrid matter of other descriptions, into the blood. But, it could neither have been the small-pox, which the doctor must long previously have passed through, and is incapable of affecting the same person more than once; or the plague, which, not being contagious, cannot be propagated by inoculation.

He next, in the month of August, under the

protection and patronage of the Greek Prince, Murusi, the Mæcenas, of Turkey, of his day, whose father was the founder of that institution, entered the Pest Hospital, near the Seven Towers, in which I afterwards conducted my experiments. Here, whilst meditating inoculation, with pestilential matter alone, he was attacked with the plague.

“ He describes this Christian infirmary,” says the New York Medical Repository, (Vol. 18, p. 363.) “ as being wretchedly constructed. The rooms in which the sick lie are narrow, badly aired, and more resembling the cells of a prison, than the chambers of a house of charity.” (This description is rather high coloured; but possibly the best wing may have been since added). “ The place has neither physician, surgeon, nor apothecary.” (There was a Turkish surgeon in my time: but I caused him to be dismissed, having found that he plotted with the Papa Parthenius, for the purpose of marring my experiments). “ A lady doctress prescribes for the patients.” (This lady I neither saw, nor heard of). “ The diet is most rigorous; meat, soup, and wine, being denied them.” (Nothing whatever, but rice water, was allowed the patients, whilst I was there). “ The greater part die,” (probably seldom less than nine in ten; and, sometimes, not above one in twenty, or thirty, survive; just sufficient to serve as decoy birds to fresh victims) “ and

they are hurried away to the grave before the breath is fairly out of their bodies." (During my time, it was impossible to know whether the patients might not even have been buried alive, since it was never thought proper to announce their death, until *after* their burial). "The proportion of women to men in this hospital, is as one to six" (The proportion varies greatly according to circumstances. Generally, if not always, the number of women affected with epidemic diseases is much smaller than that of men, as I have shewn to have been uniformly the case in the recent pestilential maladies of Spain; a circumstance depending upon their being less exposed, than the other sex, to the principal cause of pestilence¹). "In this dire abode, he went through the *natural* plague in the month of August." (Dire indeed! the word "*natural*" is here inaccurately employed, on the erroneous supposition, that an *inoculated* plague can take place). "Shocked with the scenery and acting in this asylum, he quitted it, and procured a private lodging." (This abstract does not inform us whether he met with as much difficulty, and delay, in being removed, as I experienced, in 1815). "He appears to have taken opium occasionally, in doses varying from six to eighteen

¹ Amongst my patients, in 1815, there was only one female.

grains, and asafoetida from twelve to twenty-four. He also took coffee and brandy, as usual, with lemonade for drink." (Empirically, a clumsy mode of excitation is the practice, as far as any is employed, instinctively pursued in the Levant; but never with much benefit, because it cannot be raised to a level with the intensity of the disease; besides the ignorance which universally prevails, respecting the principle of varying the remedies, according to the nature of the organs principally affected. Dr. Valli, if there be truth in what was stated to me, by some of his medical acquaintances, in the Levant, was well armed, and accompanied by confidential servants of his own. What his Journal says upon this subject, if it notices it, is not mentioned in the abstract, from which I have taken most of these particulars. With respect to myself, it would have been as impracticable for me to have procured lemonade, as a warm bath, both being attended with some trouble. He is stated to have declared, to his medical friends in the Levant, that, besides the negative mode of acting by starvation, attempts were made, in this hospital, in a more direct manner, against his life. I am ignorant whether he touches upon this in his Journal. But I can have no doubt of the probability of the fact, notwithstanding his having been armed, and the additional check that might be imposed by the

avowed protection of the then all-powerful, benevolent, and enlightened, Prince Murusi).

It would be unnecessary to detail the other experiments of Dr. Valli, as they appear to have scarcely had a determinate object, and were not followed by any useful results. Undoubtedly, as has been well observed by the American critic, commendation is due to his disinterested spirit, his benevolent intention, his undaunted perseverance, and his indomptable zeal. But, in as far as the principles promulgated in this work, may be found to approach correctness, his ill-directed labours must have been injurious, rather than useful to the interests of philanthropy, and of medical science. In his Journal, Dr. Valli disclaims all pretensions of having made a journey to Constantinople to establish a certain method of cure for the plague; and professes his great object to have been, *to ascertain whether inoculation for it could be practised with advantage*. The doctrine of contagion, upon which all his speculations were founded, being now proved to be without the smallest foundation, the whole of the superstructure raised upon it must of necessity fall to the ground. But, in proportion as his belief in contagion, and his ignorance of the means of cure, were perfect and sincere, so must his courage be admitted to have been exalted.

The list of remedies, contained in the latter

part of Dr. Valli's Journal, as blood-letting, vomits and purges, courage, diet, fires, brimstone, gunpowder, odours, vinegar, nitric and muriatic vapours, oil, fumes of indigo, and flight; together with the inertness of his proceedings, when attacked with the yellow fever, by which his existence was unfortunately terminated, at the Havannah, afford sufficient proofs, that he had no competent knowledge of the principles of cure of acute diseases; and, that he was consequently not the best qualified for the successful prosecution of the arduous investigation, in which he had so benevolently embarked.

Quitting Constantinople, Dr. Valli travelled into Asia Minor, and Syria, with a large retinue, still under the patronage, and, as I have been assured, at the expence of the munificent Prince Murusi, for the purpose of making experiments upon the epidemic diseases of animals. These experiments being prosecuted upon the same ungrounded data, as he had assumed, respecting the epidemics of men, necessarily terminated in a similar failure, and disappointment. During this expedition, the doctor, and his attendants, met with much obstruction, being treated as spies, by one of the unceremonious pachas of the country, through which they travelled; a fact, of which I was assured by a person, who had been in his suite, and, who, during my residence

at Constantinople, was actually in the service of Sir Robert Liston.

The conclusions, contained in Dr. Valli's Journal, are either trivial and useless, as to practical purposes, or such as might have been deduced from the most obvious laws of life, and could have required no new experiments to have ascertained.

1. The cow-pox does not act upon a person sick of the plague, Osserv. xx. p. 50. 2. The small pox has no operation upon a patient labouring under the plague, Osserv. xxii. *ibid.* 3. When the plague has become rife, and has assumed the character of a reigning epidemic, the small-pox is wholly unable to check it, p. 51. 4. The Greeks and Franks, who turn pale at the very name of the plague, are much more susceptible of it than the Armenians, who have very little dread of it, and the Turks, who calmly take it as it comes, p. 81. 5. The inoculation extends its protection no farther than the season in which it is performed, and does not give security to succeeding years and epidemics, p. 82. 6. The plague is a distemper so proteiform and multifarious, that he cannot assign to it a diagnostic character; inasmuch as it counterfeits all distempers, and each pestilence requires its own particular mode of treatment, p. 115. 7. He is convinced, nevertheless, that the variolous constitu-

tion of the atmosphere, and of the human frame, both have a tendency to keep away the plague, and to render it milder when it makes its appearance.

Dr. Valli, notwithstanding the little success of his experiments on the plague, determined to pursue the same career of investigation, in respect to other epidemic affections. For this purpose he went to the Havannah, where he arrived on the 7th of September, 1816; sickened of the yellow fever, on the 21st, and died on the 24th of the same month. From the communication of Dr. Frost, to the editor of the New York Medical Repository, on the subject of his sickness and death, dated the 13th of October, 1816, it clearly appears, that Dr. Valli had, from the period of his arrival in the West Indies, daily exposed himself to the operation of the causes, which usually produced yellow fever; that he was not inured to the climate; that it was the usual epidemic season; and that he was constitutionally, and from his manner of living, in a state little capable of resisting the causes of an epidemic¹. It happened, however, that, a very few hours before he was seized with the malady, he had been making the experiment of rubbing his skin with a shirt, which had belonged to a sailor, who had

¹ Dr. Frost says, that he was literally a walking skeleton.

just died of the yellow fever, and was taken from the dead body. And, it does appear, from his conduct and declarations, during his illness, that he was impressed with the belief of its being contagious; and, that, from the moment of attack, he despaired of a recovery. Nor does it appear that any treatment, promising efficiency, was employed. Could any arguments be necessary, after having shewn that no epidemic disease can ever depend upon contagion, to prove that an individual case has not arisen from such a source, the proof would be found, with respect to Dr. Valli's illness, in the circumstance of the shortness of the period between the application of the sailor's shirt, and the appearance of the phænomena of the disease, being but a very few hours. For, we know of no *general* contagious malady, which is capable of manifesting itself so immediately after the application of its appropriate virus. But to enter into farther discussion on a point already so amply illustrated, would be, at least, superfluous. I have, already, shewn the fallacy of experiments of this kind. If a person be inoculated, or rubbed with the matter of a disease, supposed to be contagious, under exposure to a pestilential atmosphere, and a disease ensues, how is it possible, from such experiment, to decide, whether it be the effect of the inoculation, or of the air?

The zeal of Dr. Valli, for professional researches, has long been eminently conspicuous. I have been assured, by the illustrious Dr. Frank, of Vienna, to whom he was well known, that he travelled on foot, to France, and to England, without being acquainted with the languages of these countries, in order to introduce into them a knowledge of the experiments of Galvani and Volta ; and that he has been known to engage in the most menial employments, in order to facilitate his scientific pursuits.

At a general meeting of the Economical Society of the Havannah, held on the 22nd of November, 1816, an eulogium on Dr. Valli was delivered by Dr. D. Thomas Romy, Honorary Physician to the Royal Family, &c. In this encomium the orator takes occasion to remark that Dr. Valli perished in the course of his researches concerning the yellow fever, like Pliny, in exploring Mount Vesuvius ; and regrets that the society, which he addresses, had not received him as a member of their body, previous to his being snatched from them by a premature death. He proposes to make up for this omission by placing his portrait in the public library ; an honour, which has yet been bestowed only on a few. Dr. Valli had been President of the Virgilian Academy, of Mantua, and of the Medical Society, of

Venice, &c. The oration concludes with this epitaph :

AQUI YACE

EL DR. EUSEBIO VALLI,

Victima de su Amor a la Humanidad.

LA SOCIEDAD ECONOMICA DE LA HABANA

RECOMIENDA SU MEMORIA,

EL DIA 10 DE ABRIL DE 1816.

CHAP. XXXVI.

EXPERIMENTS AND DEATH OF MR. VON ROSENFELDT.

IN the two preceding chapters, we have seen Dr. Whyte, entertaining a correct *opinion*, respecting what the plague does *not* depend upon, but without any *proof* of the validity of this opinion, or any adequate knowledge, either of the true cause of the disease, or of an efficient mode of treatment, fall an easy prey to the first attack of the malady; and Dr. Valli, full of the prevailing prejudices respecting the cause, and apparently ignorant of all efficient means of cure, experience the same fate from an attack of the yellow fever. These gentlemen undertook a regular system of investigation; possessing at least the pre-requisites of the usual medical education. But the most extraordinary attempt is that which I am now to consider. Mr. Von Rosenfeldt, a German, who does not appear to have had any medical knowledge whatsoever, ventured, last years, (1816, 17) to enter upon a similar enquiry. From his choosing the Pest House at Pera, for the theatre

of his experiments; and from the other precautions, which appear to have been employed in respect to him, such as his being watched by Dr. Burghardt, while there, I am led to infer that he must have profited; in so far, by my experience in the hospital of the Seven Towers, which he might possibly have been enabled to do, by the perusal of my Report to Sir Robert Liston, of which several copies had been left at Constantinople. At any rate, sufficient traces of my proceedings, the preceding year, must have remained in the recollection of the Frank inhabitants of the Levant, to supply those who followed me in the same course of investigation, with some useful instruction. It seems, however, from the results, that the lessons must have been very insufficient: and this renders it the more necessary that the present exposition should be full and complete.

The experiments of Mr. Von Rosenfeldt, as having been detailed in the public journals, with extravagant eclat, and having been made by others vehicles for the support of the prevailing delusions, respecting the cause of epidemic diseases, seem to require particular consideration. And perhaps it will be the most efficient mode of proceeding, first to give the accounts, which have been published, of those experiments, and then to comment upon them.

“*Constantinople.*—The experiments made by

Dr. Valli, a physician of Mantua, and the English Dr. Maclean, for inoculating with the plague, have been surpassed by the discovery of Mr. Von Rosenfeldt, a German, who has for some time been a resident here. The truth of this assertion is proved by the following letters, written by Dr. Burghardt, who assisted Mr. Von Rosenfeldt in his experiments. The first letter, dated the 13th of December, is to the following effect:—

‘ Early this morning Mr. Von Rosenfeldt waited upon me, and gave me notice that he was ready to begin his experiments in the Greek Hospital. Considering the serious nature of the business he was about to undertake, I was astonished to find him in most excellent spirits. I informed him of my readiness to lend him all the assistance in my power. I was occupied the whole of the morning in making preparations for this experiment. At ten in the forenoon I requested that he would make use of the Turkish vapour-bath, for the purpose of ascertaining whether or not he had been rubbed with oil, and likewise by opening the pores of his body, to bring him in such a state to the Plague Hospital as would facilitate the communication of infection. This measure appeared to me the more necessary for obtaining satisfactory proofs for and against his treatment, as these vapour-baths are known, from experience, to be the most dangerous conductors of

the plague. From the bath I conducted him to the Greek Hospital at Pera, which contained about twenty persons afflicted with the plague. I then desired him to perform, in my presence, some proof of the efficacy of his treatment, to which he gladly assented. The patients were unable to leave their beds, and as I dared not proceed further, I proposed, that he should be conducted by the director of the hospital, and the priests. The director and the priests afterwards informed me, to their utter astonishment, that he remained for several minutes in close contact with two patients, who were in a dreadful stage of the disorder, and that he even touched them, and conversed with them without the least apprehension. Having recommended him to the priests, and the director, I abandoned him to his fate, at the same time expressing the sincerest hopes, that he might not be deceived in his expectations.

“ The second letter, dated the 16th of Dec. is as follows:—‘ I visited Mr. Von Rosenfeldt yesterday at noon, and can state the following particulars respecting his plan of inoculation for the plague. The two patients whom he visited on the 10th, and of whom I made mention in my first letter, died on the 12th, being two days after Mr. Von Rosenfeldt was with them. Mr. Von Rosenfeldt, as well as his interpreter, who had

been inoculated on the 9th, in my presence, touched with their bare fingers the open pustules of some inoculated patients, who had been brought from the hospital for that purpose. Both he, and his interpreter, handled the sick persons with such confidence as to excite the wonder of the director of the hospital. Mr. Von Rosenfeldt still continues to visit the patients, and has even taken into his service a boy who has open pustules on various parts of his body.'

" *Constantinople, Jan. 25, 1817.*—The following letters of Dr. Burghardt (who conducted Mr. Von Rosenfeldt into the Lazaretto, and observed him there) give the continuation of the accounts of Mr. Von Rosenfeldt's experiments with his preventative of the plague, and the unfortunate result of them.

" *December 27, 1816.*—Having been informed by the dragoman of the Porte, that Mr. Von Rosenfeldt, before he visited persons infected with the plague, (which he did regularly twice a day) rubbed himself with oil, I immediately went to him, and soon discovered that this report was set on foot out of envy, by a person to whom Rosenfeldt would not communicate his secret. More certainly to controvert this suspicion, I caused Mr. Von Rosenfeldt, in my presence, to wash his hands with soap, to dry them, and then, also in my presence, to touch fresh plague ulcers, which

he showed himself perfectly willing to do. He touched upon several patients six or seven pustules, and as many buboes, and even put his fore finger so deep into the buboes, that the pus ran down upon his hands, which he then, with the palms of both hands, rubbed in on his bare arms, till no trace of moisture was visible. I remained fifteen minutes with him after this operation, in order fully to convince myself that the pestiferous matter must in that time have been wholly received by the absorbent vessels, into the system. I followed the same process with the dragoman, whom he had also provided with his preservative.

“ *January 12, 1817.*—Mr. Von Rosenfeldt, as well as his dragoman, found themselves perfectly well after the trial made on the 27th of December, as described in the preceding letter; and as no change has appeared in his health up to this day, (being the 16th day) his preservative may perhaps be considered as thereby confirmed, or, what perhaps is more probable, he is not susceptible of the plague, or at least is not at present so disposed as to be infected by it. In my opinion, all depends on this circumstance, and repeated experience has confirmed this opinion. Thus, for instance, we see people, who, during the most malignant plague, have served for years in the hospitals, as attendants on the sick, without being infected, and yet fall at last victims to the conta-

gion, without any satisfactory reason appearing, why they should be *infected* just at that moment, and not years before. Physicians, who are inevitably exposed to the *contagion* every year, sometimes remain untouched, till at last one or other of them is seized with the plague. Fathers of families, who have attended their family in the plague, remain uninjured, and yet are *infected* (as numerous examples prove) months or years afterward, *by other means*. This temporary susceptibility can be explained, perhaps, only by decisive circumstances, such as, irregular way of life, uncleanliness, repressed passions, which render persons very *susceptible of the plague*, and easily *produce infection*.

“ *January 21.*—Sir, with heartfelt grief, I have to inform you, that the day before yesterday, (the 19th) Mr. Von Rosenfeldt complained of vertigo, and a slight head-ach: this increased very much till noon, and his tongue was quite white: towards one o’clock P. M. he felt an inclination to vomit, which however did not take place; but toward three o’clock, he was seized with a violent diarrhœa, which weakened him so much, that he was soon obliged to lie down. He had a fever through the night, and was at times delirious: the diarrhœa became more violent, and his weakness increased in proportion. When I desired to speak to him yesterday morning, I learned

with affliction, that the unfortunate man, by his heroic zeal in the service of humanity, had himself become the victim of the scourge which he sought to avert. Two hours before my arrival, a plague-bubo appeared under the left arm pit. His mental faculties abandoned him only for moments, he was fully aware of his situation, which excited in him a kind of gloomy affliction, which was expressed in his gestures. The symptoms of the disorder remained the same during the day (the 20th), but in the course of the night preceding the 21st became more violent: the disease reached the highest degree, and all the symptoms began to be mortal. When I visited him this morning early, he was already near his end; he was no longer able to swallow the medicines. Towards noon, the body began to turn blue, and lost its natural warmth, and about two o'clock in the afternoon, the last spark was extinguished of the life of a man universally lamented by all, who even after his death will retain a grateful remembrance of his generous, but unsuccessful enterprise.

“Mr. Von Rosenfeldt had been thirty-eight days in the Lazaretto. In his last moments he had the spiritual assistance of the worthy Catholic clergyman, who also took down his last will, in which he recommends his necessitous family to the favor of his sovereign the Emperor Francis.”

Here we see the extraordinary phænomenon of a professed physician, acting as the guide to a man ignorant of the science, and introducing him to the dangers, from which he ought not himself to have shrunk, of a pestilential affection. The first thing the preceptor, and the disciple, do, is, in diametrical opposition to every correct rule of philosophising, to take contagion for granted, as the cause of plague, the existence of which it ought to have been the first object of their experiments to have ascertained. It is then obvious, either that the unfortunate Mr. Von Rosenfeldt, believing contagion, was the dupe of a weak reliance upon a preventive, and of those more discreet persons, who were his prompters, without taking part in the dangers incidental to such an investigation; or, that, disbelieving contagion, probably without any fixed grounds of disbelief, he meant to propitiate prejudice by pretending a preventive. But the idea of a preventive to a contagion, of which, far from knowing the nature, he was not even assured of the existence, and of which the existence has been shewn to be impossible, argues so complete an ignorance of the laws of nature, that, although we must respect the courage of Mr. Von Rosenfeldt, we cannot, without violating truth and propriety, allow to his irrational endeavours any farther merit. On the contrary, in as far as they could

be productive of any consequences, it would be mischief. Whether we take the declarations, which he is reported to have made, as truth, and believe that he really imagined he possessed a preventive, or suppose that he did not consider the disease as contagious, but pretended, in deference to popular belief, to possess the means of preventing the plague; it is certain, that, in either case, if he could have come out of the hospital, after a considerable residence, without having been affected with the disease, the credulous would have been persuaded that his exemption from the malady must have depended upon the virtues of his nostrum; and, if it were composed of saw-dust, it would have commanded an unprecedented sale, and ensured a splendid fortune. I prefer, however, supposing, since it is the most creditable supposition, that he actually disbelieved contagion, and only meant to continue the pretence of a preventive, until he should have ascertained less unphilosophical means of producing general conviction. But, even in this case, the attempt, although meritorious as to intention, would have been the extreme of folly, independently of the dangers to be apprehended from the nature of the establishment, since it appears that he did not possess, or did not imagine that he possessed, any knowledge of an efficient method of cure. It is also obvious, that, if the notion held out

of a preventive could have been realized, the trade of the persons officiating in the hospitals would have been ruined ; and, that, by such a proclamation, Mr. Von Rosenfeldt was offering a reward for his own destruction, or, in a more literal sense, rendering that catastrophe inevitable.

Mr. Von Rosenfeldt, having, it appears, resided for some time at Constantinople, must have been, to a certain degree, inured to the climate : the state of the air, whilst he was in the hospital, was not intensely pestilential : and, if his constitution was of an ordinary vigour, (but respecting this I have no information,) and he had been under the usual circumstances of life, with respect to diet, and tranquillity of mind, it is more than probable that he would not have been affected with the malady. But, however inured to the climate, however robust his constitution, however slight the noxious qualities of the atmosphere, and however fearless his mind, it is notorious, that the executive agents of the Pest Houses well know how to apply those adventitious causes of pestilence, consisting of negation of nourishment, and irritation of mind, in such a manner, as, however slightly the proper causes might be operating, and independently of a more active interference on their parts, to produce, in due time, a certain accession of plague. If that accession were not mortal, they had still the more

active interference in reserve : and, how could Dr. Burghardt have known, that, in Mr. Von Rosenfeldt's case, it was not actually put in force? It is true that the disease might, of itself, have appeared to promise a sufficiently certain death, so as to render such interference unnecessary. But, if it were otherwise, the very treatment which he must have received, from those who had the thread of his destinies in their hands, with the dread of a preventive hanging over their heads, however ill-founded that dread might be, would have rendered it their most politic, and their safest course, not to leave the tragedy uncompleted. I cannot, therefore, but apprehend, that, Dr. Burghardt, from the manner in which he treats the subject throughout, and especially the account which he gives of the state of Mr. Von Rosenfeldt, in his last moments, which, of course, he could not have known but directly or indirectly from the servants of the hospital, betrays either a total unacquaintance with these institutions, or an unpardonable wish to palliate their turpitude.

From all the experience, which we possess, on this subject, it appears, that, those who have resided, as investigators, at any of the Pest Hospitals in the Levant, have invariably been attacked with the plague, whilst their domestics and interpreters, as well as the domestics of these

institutions, have, perhaps invariably, remained exempt from the málady. Dr. Valli, Mr. Von Rosenfeldt, and myself (Dr. Whyte was at an army Pest House,) as well as the physicians in the Russian service, who were sent to the Levant to investigate the plague, have all been in this predicament. Dr. Valli survived: and although the account he gave of these institutions was very unfavourable, yet it was by no means adequate. For having had the same good fortune, I think I cannot shew my gratitude better, than by depicting the true nature of those disgraceful establishments, in order to prevent future investigators from entering within their walls, and to effect the total abolition of so inhuman a system.

Throughout the statement of Dr. Burghardt, there is something extremely contradictory, if not disingenuous. He says: "I then desired him to give, *in my presence*, some proof of the efficacy of his *treatment, &c.*" But, "*the patients were unable to leave their beds, and, as I dared not proceed farther*, I proposed that he should be conducted by the *director* and the priests." In the first place, it does not appear, from the proceedings, that Mr. Von Rosenfeldt professed any medical treatment, but merely prevention. How, then, could he have given any proof of its efficacy? In the next place, I am at a loss to

conceive, as the patients were unable to leave their beds, and as Dr. Burghardt durst not go into the hospital, how Mr. Von Rosenfeldt could possibly have made any experiments "in his presence." And, in the third place, it is altogether incomprehensible to me how Mr. Von R. should have been conducted into the hospital by one of the *directors*. Of all the directors of the Pest Hospitals, whom I have known, or conversed with, beyond the walls, none ever ventured to enter them.

"The director, and the priests," he proceeds, "*afterwards* informed me, *to their utter astonishment*, that he remained for several minutes in close contact with two patients, who were in a dreadful stage of the disorder, and that he even touched them, and conversed with them, without the least apprehension." The most extraordinary circumstance here is, that priests, who have themselves been, probably for years, in daily or hourly contact with pestiferous patients, with impunity, and who, if they were not a new generation of priests, must have seen myself, only sixteen months before, in contact with all the sick of the Pest Hospital of the Seven Towers, six times a day, frequently continuing among them for an hour together, should have been represented as feeling utter astonishment at occurrences so little new to them.

In another place, he says: " I caused Mr. Von Rosenfeldt, *in my presence*, to wash his hands with soap, to dry them, and then, *also in my presence*, to touch fresh plague ulcers, which he shewed himself perfectly willing to do." In this language, there is, at least, an ambiguity which ought to have been avoided. It is, indeed, possible, that the servants of the hospital might have been prevailed upon to bring the plague patients beyond the gates, and that the sick might have been prevailed upon to leave their beds for that purpose ; and that Mr. Von Rosenfeldt might have made those experiments " in the presence" of Dr. Burghardt, although at what is deemed the proper infectious distance. All this, I say, may be possible, although neither the sick, nor their attendants, shewed any such alacrity in my time : but the circumstances, if they happened, ought to have been distinctly specified. It is, however, wholly immaterial to the main question.

Dr. Burghardt, it will be observed, gives an account of the situation of Mr. Von Rosenfeldt, during his illness, as if from personal observation, although I must presume, from what I have myself experienced, that this could not have been the case. Indeed, on the 21st of January, 1817, he only says that he *visited*, not that he *saw* him. It is certain that he could not have seen him after he was confined to bed, as he

durst not have entered the house, even if his own inclination had led him to disregard the supposed danger, for fear of being obliged to perform quarantine.

There are other minor incongruities in this statement, upon which I hold it unnecessary to comment.

Respecting the question of contagion, although it is almost superfluous to say one word upon the subject, in reference to this case, yet I may observe, that it is quite ridiculous to suppose that a disease, depending upon such a cause, would abstain for thirty-eight days from making its attack, the cause being daily applied, and then all at once commence its operations, and finish them in the short space of two days. Contagion is never so tedious in its attack, or so quick in its issue. It never kills in two days.

Nor is it even probable, that the air of the place, the effects of improper living, anxiety of mind, and all the other real causes, proper or adventitious, of plague, could, in such a situation as the hospital at Pera, have suddenly arisen to such a degree of intensity, as alone to produce this rapid catastrophe. At any rate, I am fully persuaded, that the adventitious must have formed a much greater proportion, than the proper causes, towards the production of the disease; and that, if any thing had been wanting to ensure

death, under Mr. Von Rosenfeldt's unwise boast of a preventive, it would not fail to have been supplied.

I not only do not hesitate to pronounce this opinion, in the most unqualified manner, because there can be no delicacy due to monsters, who are enabled, by the credulity and fears of their countrymen, to make of plague a horrid and a gainful traffic; but I even think it my duty to proclaim it, as a warning to all future investigators.

I should have thought it strange that the resident physicians of Constantinople, as they could scarcely have been unacquainted with the intentions of Mr. Von Rosenfeldt, before he entered the hospital, should not, from their perfect knowledge of the treatment experienced by Dr. Valli and myself, at these dépôts, have dissuaded him from encountering so certain a danger, had they not acted precisely in the same manner towards myself. After they saw me return in safety, indeed, they kindly reproached me for not having informed them of my intention, (which, by the bye, as a matter of general notoriety and conversation for weeks, could not have been unknown to them) since they could have given me such intelligence respecting the place, they said, as would have infallibly altered my plan. One of them, an Italian, observed with a sneer, from whence I

concluded, that the medical faculty in Turkey, who do not themselves visit plague patients, are not quite exempt from uncordial feelings towards those who are not such strict observers of professional etiquette :—" Vous etes, donc, venu, Monsieur, pour nous donner un leçon." " Non, Monsieur," I replied ; " mais pour vous en recevoir."

By the death of Mr. Von Rosenfeldt, the public would have lost all chance of benefiting by his discoveries, if he should have made any ; for, besides that no Christian in the Levant would touch the papers of a person, who had died of the plague, or had been at all near plague patients, until they had undergone fumigations, and lustrations of every kind, they would, in the first instance, fall into the hands of the servants of the hospital, who have an obvious interest in suppressing or appropriating them ; and, from their situations, would be enabled to do so, with secrecy, or impunity. The chiefs of these departments are generally Monks. The instinctive antipathy of Monks to knowledge is proverbial. Nothing can be more true than what was wittily observed of these gentlemen by Lord Orford, better known in the literary world as Horace Walpole, that, whenever a ray of light breaks into their cell, they are ready to cry fire. Those of the pest hospitals, besides the deep stake which they possess in ignorance

generally, have a particular interest in perpetuating that which relates to the cause of epidemic diseases. When Mr. Von Rosenfeldt, therefore, after having been above thirty days exposed to pestiferous matter, without being affected with the plague, had invited the foreign ambassadors to see him perform his experiments, and fixed a day for the purpose, he had irrevocably sealed his own doom. He was dead before the day appointed arrived!

From the best consideration, which I have been able to give to this case, it appears to me quite evident, that Dr. Burghardt could have known nothing of the treatment of Mr. Von Rosenfeldt, after his illness, or of the circumstances of his death, but from the reports of the servants of the hospital; and, that, to state them as from his own knowledge, was, if not to delude the public, at least to allow them to fall into error.

It has, I think, been rendered sufficiently clear, by the various cases which I have here adduced, that it is utterly unsafe to attempt to investigate the plague, in the Levant, but in the independent manner which I proposed to the Ottoman government; as well as that there is no probability, unless by the intervention of foreign influence, or great political changes, in Turkey, that such an opportunity will ever be accorded.

CHAP. XXXVII.

Information to future investigators—delusive results of the experiment of inoculation for plague—inconsistency of it, more especially in those who believe in contagion—diseases that kill in one or two days cannot depend upon contagion—monstrous assertion that pestilential matter can act as a preventive against its own supposed effects—qualifications necessary to investigate the plague with safety—reasons of the failure of investigators—failures more mischievous than abstinence from enquiry.

THUS have I endeavoured, by an elucidation of the successful results of my own experiments, as well as of the unfortunate issue of those of my fellow-labourers, to inform future investigators respecting the obstacles which they will have to encounter; and, by explaining circumstantially the nature of these obstacles, to teach them how to avoid dangers that are, and to vanquish dangers that are not, insurmountable. In thus presuming myself qualified to offer instruction, upon a matter so delicate and important, it is right to observe, that I do not found my pretensions so much upon the particular results of my experiments and observations in Turkey, as upon the fruits of many years antecedent experience and

contemplation of epidemic maladies, in various quarters of the globe, as well as of much assiduous study, and laborious historical research. Or rather, let me say, my pretensions to a knowledge of the subject, are founded on the united results of a continued series of investigation, from an early period of life, down to the present moment.

It has been shewn, both by facts, and by reasoning, that the experiment of inoculation, besides being useless, or supererogatory, is fallacious and misleading. Thus, Dr. Whyte, after passing through several inoculations with plague matter, did not take the disease; and, if he afterwards caught it, at the period of another inoculation, as is stated, the inference in fair reasoning would be considerably in favour of this seizure having been only an accidental coincidence. Mr. Von Rosenfeldt was thirty-eight days exposed to the action of pestiferous matter, before he was attacked with the malady of which he is stated to have died. In this case, the existence of contagion, which is, or ought to be, the object of enquiry, being, as usual, taken for granted, endeavours are made to account for the disease not being produced for so long a period, under a constant exposure to its presumed cause, by the silly doctrine of susceptibilities. This mode of begging a question ought to be wholly banished from science.

Before finally dismissing this part of the subject, I shall, in order to place in the clearest possible light the absurdity of the ideas entertained concerning it, give two more extracts from the public journals, with suitable comments.

Constantinople, February 15.—Mr. Von Rosenfeldt, who had attracted universal attention by the inoculation of the plague, after having happily passed thirty-eight days in the Lazaretto without any accident, was attacked by the disorder on the thirty-ninth day, and on the fortieth fell a victim to his exertions in favour of humanity.—(*Hague Courant*, March 5.)

Paris, March 9.—M. Vassali Eandi, and other Italian physicians, who have tried, during twenty years, on their own persons, the same inoculation, have been more fortunate, and imagine that they have experienced from it a preservative effect, since they have been able to visit with impunity for a length of time the pestilential hospitals.

The edition, in the first of these articles, differs somewhat from that of Dr. Burghardt, in respect to the duration of the disease, by which Mr. Von Rosenfeldt is said to have perished. The difference, indeed, is not material, excepting in degree of absurdity : for, if a disease, that kills in two days, cannot depend upon contagion, it would be still more ridiculous to suppose, that a disease, which kills in one day, could arise from such a source.

But, as if this were not a sufficient trial of human credulity, the common sense of mankind is, in the article which follows, insulted in a still higher degree, by the palpable and disgraceful falsehood, that this species of inoculation has been found to produce, *in certain Italian physicians*, a preservative effect. According to this account, what is *death* to a *German* is *preservation* to an *Italian*! That the physicians mentioned, or alluded to, may have attended Pest Hospitals, with impunity, is quite credible; and the reason quite obvious, if these hospitals be situated in good air. But to affirm that their preservation should have been produced by the very agency, which is alledged to be the cause of the disease, is a contradiction too monstrous to impose even upon the meanest understanding, when subjected to the slightest examination.

Besides having shewn the impracticability of successfully investigating the plague any where, by the means which have been usually resorted to for that purpose, or of rendering success palpable, by the best and most appropriate means, when employed in the depôts, which have been established for these maladies in the Levant, I have also, I trust, succeeded in rendering it manifest, that, notwithstanding these very formidable obstacles, I have happily been enabled to bring this investigation, both with respect to the cause,

and to the cure of these maladies, to the most gratifying and satisfactory issue.

It now only remains for me to state the qualifications, that are necessary, to enable others, with personal safety, to repeat the experiments, by which I have obtained my results, whether with a view to their verification, or their application, upon an extensive scale, for the benefit of communities.

1. The first and most indispensable qualification, for the successful repetition of experiments on the plague, is undoubtedly a knowledge of *the means of cure*; a knowledge, which is indispensable to ensure recovery from any severe attack of the malady, and would be equally necessary to ensure personal safety, whatever might be the *cause* of the disease.

2. It is necessary that the investigator should not begin by taking for granted the question, respecting the cause of epidemic diseases, to resolve which is a principal object of his enquiry: for, the assumption of an erroneous cause, which must be the unavoidable consequence, would render success impossible.

3. In order to avoid an attack of pestilence, and to enable him to apply the fruit of his knowledge to the cure of other persons, it is necessary,

a. That he should be acquainted with the *real* cause of such diseases;

b. That he entertain no dread of the disease, beyond what may be inspired by any other severe malady.

c. That he choose a situation, in which the air is pure, for treating persons ill of the plague.

d. That, whilst exposed to a pestilential atmosphere, in whatever degree, he do not live low, and be dispensed, if a Catholic, from keeping Lent; but that, on the contrary, he live rather high, but regularly, avoiding vicissitudes, and extremes of every kind.

e. That he be, to a certain degree, inured to the climate.

f. That he take due exercise.

With the knowledge here inferred, and a correct observance of the rules mentioned, I feel warranted, by my particular experience, in asserting, that there can be little, or no danger, in the treatment, or investigation of the plague; but, that, under these circumstances, those, who may be ambitious to distinguish themselves, by improving medical science, and relieving the sufferings of their fellow-creatures, may, without apprehension, give free scope to the noble enthusiasm of their minds.

Let us apply these principles, toward accounting for the failure of those who have hitherto attempted to investigate this subject. Amongst these I do not think it necessary to include the

authors of works, who have been only casual observers of particular epidemics, as the medical officers of the British and French armies in Egypt; or of the allied and French armies in Spain; or of practitioners in civil life, who have only detailed the occurrences they have met with, in the course of their ordinary avocations. The proceedings of those, who have entered upon a regular course of researches, as professed investigators, as the three gentlemen, whose experiments have been detailed in the preceding chapters, I regard as alone worthy of particular consideration.

Respecting the first essential, Drs. Whyte and Valli, appear to have been so much imbued with the ordinary medical notions of bleeding and purging, that, if there be any truth in the principles here maintained, respecting the cure of diseases, they must have been, under every circumstance, inadequate to the efficient treatment of a severe pestilence: and of Mr. Von Rosenfeldt, it is not even pretended, that he possessed any of the most common-place medical ideas. This disqualification, of which their unfortunate catastrophe afforded additional proof, would, of itself, have formed a sufficient cause of the failure of their researches.

In investigating the cause of the disease, Dr. Valli and Mr. Von Rosenfeldt fell into the com-

mon error of setting out by implicitly taking for granted, what ought to have been the first and principal object of their enquiry ; and Dr. Whyte, although he saw reason to doubt, or disbelieve, the existence of contagion, founded his disbelief upon inadequate grounds, and fell into a delusive course of experiment. Consequently, they could not fail to have missed ascertaining the true prophylactic means against the malady. They were severally led, but upon different grounds, to the same train of research. They all subjected themselves to some kind of inoculation ; and this circumstance draws, in this respect, a line of distinction between these investigators and myself. The fallacy of this experiment, which seems to have constituted the sole ground of their reliance for the chance of a discovery has been already shewn : and their failure to ascertain whether contagion be, or be not, the cause, as well as what is the actual cause, of plague, has been a matter of course.

Two of these gentlemen imagined, or pretended, that they possessed preventives. That of Dr. Valli was small-pox ; that of Mr. Von Rosenfeldt remained undivulged. To those, who understand the laws of life, and especially of epidemic diseases, it must be obvious that all preventives, excepting the absence of their true cause, an impure atmosphere, &c. must be imaginary or

feigned. Hence, whenever a noxious atmosphere, and the other concurrent causes of pestilence, happen to be applied to the investigator, with due intensity, the disease will certainly follow ; the reputed antidote will be disgraced ; shame and vexation, if the malady should have been originally mild, will render it mortal ; and the multitude will cling, with increased earnestness, to their original belief.

With a laudable, but not very judicious partiality for their country, the credit of originality, in this frivolous experiment, has been claimed, by French writers, for Baron Desgenettes¹. In him, Dr. Valli, and Mr. Von Rosenfeldt, as implicit believers in contagion, to inoculate for the plague, was at least inconsistent ; since, as the disease is notoriously capable of affecting the same person repeatedly, if it were possible that it could also be propagated by contagion, this operation could have no other effect than to spread the disease. In the hands of Dr. Whyte, who was not a believer in contagion, the experiment, if not so inconsistent, was scarcely less unwise ; because, if, under certain circumstances, it should even be possible, so to conduct this experiment, as to disprove the existence of contagion, it was, however, impossible, under those in which he made the attempt, so to act, as

¹ Literary Gazette, No. 8, p. 16, and other public journals.

that, if disease followed, the effect of a pestilential air should not be confounded with that of a supposed contagion ; or, that, if no disease ensued, the nature of the original malady should not be called in question.

This alledged inoculation of Baron Desgenettes¹ seems to have been regarded, both by himself, and by others, as a political, rather than a medical measure ; and, in the former view, it might have been both wise and expedient, although in the latter, it was at least useless. Bonaparte, placing himself in contact with a plague patient, in his military hospital in Egypt, has formed the subject of an interesting painting. The object of both was to give confidence to the army ; and it was, to a certain degree, attained. Dr. Whyte, however, had, by far the best ground for his proceeding ; and he had at least an equal originality : for, if his experiment was even subsequent to that of Baron Desgenettes, there is no probability that the one was an imitation of the other, since the latter could not have been published, or generally known, when the former was made.

¹ If Baron Desgenettes really inoculated himself with plague matter, it is strange that he should not have been convinced, by the disease not following, that it does not depend upon contagion. This incongruity, the fact being admitted, appears to have been, as usual, accounted for, by the convenient doctrine of susceptibilities.

But it is highly probable, that the credit, such as it is, of having been the first to make the attempt of inoculation for the plague, is not due to either of these gentlemen; but that the experiment had previously been made, or at least suggested, by other persons¹. The real cause of epidemic diseases, being now distinctly ascertained, the world will not be very solicitous to learn what country gave birth to the person, who had been the first to make this inconsequential experiment. For, in an investigation of such extraordinary importance, time will not fail to separate, what is only glittering, from what is solid, in the enquiry.

Upon the whole, it appears, that, of these three investigators, not one was acquainted with an efficient method of treating the disease, of which it was a principal object of his enquiry to ascertain the nature, and the cause; and, that, consequently, upon their being themselves attacked with the malady, in a severe degree, they easily fell victims to its violence.

Two of them, Dr. Valli, and Mr. Von Rosenfeldt, were subjected to the operation of the adventitious causes, consequent on the belief in

¹ This, although I do not know of any particular instance, I am led to conclude, from recollecting, that Olivier, who had written before the French and English armies went to Egypt, deprecates this experiment, as at least useless.

contagion, as well as to starvation, and irritation of mind, in the Pest Hospitals. Dr. Whyte was probably only subjected to the operation of its proper causes. They were all three deficient in knowledge of the true cause, and means of prevention, of the malady. Consequently it was next to impossible, that, under the constant operation of its real cause, they should not be seized with plague; or that, being seized, they should not, if severe, fall victims to its ravages. But, failures, under striking circumstances of this kind, are more injurious to the cause of science, not simply among the multitude, but even among persons the best informed, than, if no attempt at investigation had taken place.

CHAP. XXXVIII.

Departure from Pera—characteristic incidents respecting the plague, at Basilika and Messevria—arrival at Warná—at Bucharest—interview with the Prince of Wallachia—information respecting the plague of 1813 at Bucharest—copy of a Letter from Sir Robert Liston to Lord Castlereagh, respecting my investigation—remarks on a paragraph of that Letter.

ON Thursday, the 26th of October, I left Pera, accompanied by Mr. Black¹ and Dr. Macguffog, who agreed to escort me to the mouth of the Bosphorus; on the morning of the 27th, having slept the preceding night at Buyukderé, entered the Black Sea with a fair wind; on the 31st arrived at Warná²; and, on the 7th of Novem-

¹ Mr. Black, to whose attentions I was so much indebted at Constantinople, is the resident partner of the house of Messrs. Niven Ker, and Co., of London.

² I proceeded coastways, in a six oared boat, which I was assured would be a more certain conveyance than vessels of larger size, should the wind prove adverse. At Basilika, a Greek village, on the coast of the Black Sea, the inhabitants, having heard that the plague prevailed in some of the villages of Roumelia, threatened, if we did not stand off, to fire at us; and, it was not until we assured them that we

ber, at Bucharest, the capital of Wallachia. In the evening, Mr. Wilkinson, the British consul, accompanied me from the hotel Josepa, to Mr. Fleishackle's, the Austrian consul, for whom I had a letter of introduction. I here met with Baron François Sturmer, son of the Internuncio at Constantinople, on his way to Vienna. We agreed to travel together, and to set out on Friday.

Wednesday, Nov. 8th. In the evening, I paid my respects, by appointment, to the Prince of Wallachia, to whom I was introduced by Mr. Wilkinson. He received me with affability, and conversed, with great fluency, in the French lan-

did not come from any of the *infected* villages, nor had any illness on board, that they allowed us to land¹.

At another village, or rather a small town, called Messervia, my Greek boatmen were so much alarmed, having heard that there were cases of plague in the place, that they pulled off from the shore, and anchored in the road, where we remained until we resumed our voyage toward morning. Some Turkish boatmen, who happened to arrive at the same time, drew up their boats on the beach, and communicated freely, and without apprehension, with the inhabitants. The province of Roumelia, is, and has always been, very subject to the plague.

¹ Basilika, anciently the town of Sicyon, was, some centuries ago, depopulated by a pestilence; which tradition states to have been a punishment for the conversion of a Christian church into a mosque. Univ. Hist. vol. vi. p. 150.

guage, on the object of my journey to Turkey. He informed me, that, in the plague, which infested Wallachia, two years before, (in 1813, when it prevailed at Malta) three hundred persons died daily in the town of Bucharest, in the months of August and September, and, that, in the month of October, it began to decline, and, in November, ceased. No pestilence had occurred, in the province, for twenty years before. Whilst we were conversing, his body physician, a German, came in; and the Prince observed, smiling, that he was not entirely free from dread of the plague. This gentleman informed me, that he had prescribed muriatic acid internally; and I was assured, by others, that Dr. Groman, a physician then at Vienna, had prescribed phosphorus, in that malady. But it was not said with what success. It may, however, be inferred, as they all prescribed at the presumed infectious distance, that the success, as well as the regular administration of the remedies, were more than doubtful.

Relays of eight horses were ordered for Baron Sturmer and myself, at each post house, in the Wallachian territory. Prince Yanco Carrajio, the actual governor, or waywode of the province, is a Greek by birth, and was formerly head dragoman of the Porte. This is the usual course

of promotion. He succeeded Prince Ypsilanti, who went over to the Russians in the last war.

Thursday, Nov. 9th. At the Austrian consul's, at dinner, I met with two German physicians, Drs. Reiter, and Frank. The accounts which I received from them, of the mortality, and periods, of commencement, and cessation, of the plague, at Bucharest, in 1813, corresponded entirely with the statements, which were made by the Prince, on the preceding evening.

By the post just arrived from Constantinople, I received from Mr. Frere, the copy, which had been promised to me by Sir Robert Liston, of his letter to Lord Castlereagh, on the subject of my investigation; to which, in order that the whole of the subject may be before the public, I shall here give insertion.

“ Constantinople, Oct. 12, 1815. ”

“ My Lord,

“ DR. Charles Maclean, recommended to me in your Lordship's letter of the 29th of April last, arrived here towards the end of July, at a time when the plague prevailed in a slight degree in this capital; and he shewed an anxious desire to commence without delay the investigation, of the nature of the disease, and the trial of the methods of cure which he conceived likely to prove successful.

“ The only means of obtaining an immediate opportunity to carry his design into effect appeared to be, that he should get admission into one of the Greek Hospitals, with liberty to administer his medicines to the patients, and this idea, though liable to strong objections, he eagerly embraced.

“ On an application, through me, to the Turkish ministry, orders were immediately issued to the managers of the Plague Hospital, established without the walls of the city, at a small distance from the Seven Towers, requiring them to permit Dr. Maclean to prescribe such treatment of the sick as he might judge proper; a house was assigned to him adjoining to the hospital; a table was ordered to be kept for him; he was furnished with a horse for the purpose of taking exercise; and the servants of the establishment were to be at his command.

“ He found in the house several patients; he visited them frequently, prepared their medicines himself, and administered them with apparent success. The attendants at first shewed a willingness to aid him, and to render his situation comfortable.

“ But this did not last long. The servants seem to have become tired of the unusual trouble of assiduously waiting upon the patients. They came in the sequel to disobey and to neglect, if

not to insult the doctor. They prepared his food in a way that was disgusting to his palate: they even seemed purposely to withhold the articles for which he shewed a taste.

“ After a few days he found he had himself caught the infection in a serious degree, accompanied with buboes, and a periodical delirium. He apprehended that, all circumstances considered, he could no longer remain in the hospital without danger to his life. He removed to private lodgings, where he effected his cure by means of his own medicines, and he is now, after performing quarantine, restored to society.

“ Dr. Maclean has found the system of management of the hospitals so bad, the prejudices of the attendants (who are taken from the dregs of the people) so very strong, their principles and conduct so profligate, that he is now convinced no progress can be made in the cure of the disease in those public institutions; and he is determined to have no further connexion with any of them. He is willing however to re-commence his investigation; and he is confident he can, by his method of cure, preserve a great number of lives, with the aid of a *well organized establishment*, by which he understands a building suitable to the object, with two assistant physicians, one surgeon, one apothecary, one or two interpreters, and a proportionate number of inferior

agents, all at his disposal, and command. These he thinks it would be necessary to procure from England; and with that view he proposes immediately to return to London, and to come back to this place in the spring.

“ There is reason to think, however, that the Ottoman government will not consent to furnish the means of executing the doctor's plan on the scale suggested, and they have a plausible motive for declining it. The distemper which has prevailed during the late season has been of a character so comparatively benign, that an unusually small number of patients are said to have been carried off by it, in the other hospitals, as well as in the one where the doctor applied his remedies, so that they say a sufficient proof has not been given of the efficacy of his treatment to justify the Porte in appropriating to the object proposed, any considerable portion of the public funds *a considerable portion of the public funds*.

“ The Doctor has addressed to me a detailed and very able statement of his proceedings, which will of course be communicated to his patrons at home. It contains a number of interesting particulars; and the opportunities he has had of a narrow examination of the plague both in the case of the patients of the hospital, and in his own person, have given him a knowledge of the disease which may prove of essential benefit.

“ In the mean time, there is cause to apprehend, that in consequence of the expence of his journey out and home, and of his residence in this country, he will be personally a loser on the occasion, and it will remain with his Majesty's Government, and with the particular patrons of the undertaking to determine, whether the ardent zeal, the intrepid resolution, the total neglect of all personal considerations which Dr. Maclean has displayed in the course of this business, ought not to procure him some suitable reward.

“ I have the honour to be, &c.

“ ROB. LISTON.”

“ *The Lord Viscount Castlereagh, K. G.*

&c. &c. &c.”

Upon my arrival in England, I thought it right to send a copy of this letter, together with some other documents concerning the subject of my investigation, both to the East India Company, with which I had long been connected, and to the Levant Company, which have particularly patronised my undertaking: and, having afterwards learnt, that an undue impression had been made, by a certain paragraph of it, respecting the results of my researches, I thought it necessary to enter into some explanation, in a letter, which I addressed, on the 6th of March,

1816, to Jacob Bosanquet, Esq. deputy governor of the Levant Company; from whom, as well as from the noble governor, Lord Grenville, and the members of that body generally, my representations, on this subject, have always met with attention and indulgence.

The following is the paragraph to which I here allude :

“ There is reason to think, however, that the Ottoman government will not consent to furnish the means of executing the doctor's plan on the scale suggested, and they have a *plausible* motive for declining it. The distemper, which has prevailed, during the late season, has been of a character so comparatively benign, that an unusually small number of patients are *said* to have been carried off by it, in the *other* hospitals, as well as in the one where the Doctor applied his remedies, so that they say a sufficient proof has not been given of the efficacy of his treatment, to justify the Porte in appropriating to the object proposed, any considerable portion of the public funds.”

Instead of inserting a copy of the letter, which I addressed to Mr. Bosanquet, I deem it more efficient, the subject having since acquired a greater development, to embody the purport of it, with observations, which have subsequently

occurred, and thus to give the reader a more complete insight into the fallaciousness of the inferences, to which, if unexplained, the paragraph in question might give rise.

In order to obviate misapprehension, I think it right to set out, by declaring, that it is not to any part of Sir Robert Liston's letter that I object; it, on the contrary, evinces toward myself personally a disposition, which cannot but be highly gratifying to my feelings; but to the nature of the evidence, upon which one of its statements must have been founded. The statement in question is such, as Sir Robert, attending impartially to the assertions of both sides, must have felt himself called upon, in the situation in which he stood, whatever might have been his private opinion of the relative credibility of the respective testimonies, submitted to him, to have made to the British government, upon the occasion. The paragraph, upon which I feel it necessary to comment, is, indeed, nothing more, and professes to be nothing more, than a declaration of the *avowed* motive of the Ottoman government, and which Sir Robert only terms "*plausible*," for declining to furnish the means of executing my plan, upon the scale suggested, after my recovery; and, seeing the full exposition, which I have already made, in the narrative of my researches in the Levant, of the *real* mo-

tive, by which the Porte must have been actuated, upon this occasion, I should have felt it unnecessary to have again adverted to the subject, but for the extrinsic consequence, which allegations so unfounded might possibly derive, from being transmitted through the medium of the British ambassador.

The Turkish government, having, as has been shewn, very powerful reasons, for refusing to concur in any measures, which might tend to mitigate the devastations of the plague, felt themselves compelled to assign other motives than the real ones, for their rejection of my plan; and the only pretext they could find, that would not appear both indecent and inhuman, was, “that a sufficient proof had not been given of the efficacy of my treatment.” And what *proof*, I would ask every candid and reflecting reader, does he imagine, would have been considered, by the Ottoman government, *sufficient*? Was it not known to them, that, in defiance of every villainy, and every intrigue, even to the destruction of my patients, which I publicly represented to the directors of the hospital, I succeeded in effecting cures, both in other patients, and in my own person? Might not my offer of resuming the investigation, after such an ordeal, have convinced them; nay, must it not have convinced them, unless they had been pre-determined not

to be convinced, that I felt a full assurance of possessing a knowledge of the cure of the disease? And, even at the risk that my assurance might have been delusive, would not a government, that had not a direct interest against the success of the plan proposed, have thought it worth while to incur some expence, in bringing to a decisive issue an investigation, of which the benefits might be incalculable to their subjects, and of which the opportunity so seldom occurred?

But, determined that an investigation, which had already manifested unequivocal indications of success, should not be allowed to arrive at a degree of maturity, which would place that success beyond a doubt, the Turkish government were only anxious, in the eyes of the world, to throw the blame of discontinuing it from their own shoulders; and they were sensible that they could not effect this, but by transferring it to mine. Finding that they could not induce me to make the abandonment of the investigation my own act, (great pains were taken, in the mean time, to propagate the belief, that I had no intention of resuming it) they thought proper to have recourse to what they could not but have known to be downright falsehoods, as pretexts for the rejection of my propositions.

Let us, however, suppose, that they might not have been aware that their allegations were un-

founded, and that they did not themselves seek for pretexts ; but that evidence was spontaneously presented to them, tending to shew that my investigation had not been attended with success ; it will be proper to enquire a little into the nature of this evidence. It is certain that it could only have consisted of the assertions of the servants of the *dépôt* ; the atrocity of whose conduct I had so strongly depicted, in my Report to Sir Robert Liston, as well as in conversation, some weeks previous to the rejection of my propositions, by the Porte, that, if they had no other motive for wishing to stifle my enquiry, they were placed in the predicament of not being able to exculpate themselves, but by condemning me. The Turkish government could not but well know the value of, and were not to be deluded, by this species of evidence. Nor is it any great stretch of the imagination, to suppose, that, as it was in their policy, if such evidence had not spontaneously been produced, it would have been ordered to be provided.

Equally destitute of foundation is the assertion, from whence an unfavourable inference respecting the success of my treatment was attempted to be deduced, that, this season, “ an unusually small number of patients have been carried off by the plague, in the *other* hospitals, as well as in that one, where the doctor applied his remedies.”

Without being literally untrue, this is but the evasion of a direct falsehood. Whatever might have been the number of patients carried off, it is certain, that, as there was no medical treatment, in any of the hospitals, excepting in the one, under my immediate care, there could not have been any means of comparison, in respect to cures. If any patients were sent to any of the other hospitals, as it was in direct violation of an agreement which I made with the directors, that *all* the patients should be sent to that at the Seven Towers, it must have been with the fraudulent view of making an unfair comparison¹; and, of course, they would be composed of slight, or incipient cases, or convalescents; whilst all that were severely afflicted, or beyond recovery, were sent to the hospital, near the Seven Towers. The reader will recollect, as a fact in point, the case of the boy, labouring under a slight malady, who was sent away from that hospital, on one of the days that I was confined to bed.

¹ Aware of the probability that manœuvres of this kind would be resorted to, I stipulated with Mr. Stavrack, on the day of my entrance, that no plague patients should be sent elsewhere than to the hospital of the Seven Towers. That hospital was by no means full; and I never heard of patients being sent to the other hospitals, until the Porte found that my success could only be prevented from becoming immediately palpable, by their entire rejection of my plan.

With respect to the alledged benignity of the malady, I may remark that no disease can be more severe than being mortal ; and that a great majority of the cases, that came under my care, were in advanced stages, and incurable, at their entrance. In this respect, the period was particularly unfavourable to investigation. When the constitution of the air is but slightly pestilential, and but few cases of plague occur, the persons seized, where the belief in contagion prevails, think it right to conceal their disease as long as they can, *i. e.* until it is no longer curable : whereas, in seasons more decidedly pestilential, and when a great number of persons are seized, every one, who falls ill, or appears indisposed, is immediately considered as having the plague. Under such circumstances, it is neither so necessary, nor so practicable, to conceal the nature of the complaint ; and, consequently, if proper medical treatment were at hand, as it could be applied at an earlier period, it must, other things being equal, be productive of greater benefit. The efficacy of the treatment would be also more palpable to the world.

In one respect, however, the mildness of the epidemic constitution of the air was of some advantage ; as, whilst the sickness that occurred was sufficient to enable me to satisfy my own mind

of the correctness of my principles, respecting the nature and cure, and the cause and prevention, of plague, which was my immediate object, the risk to myself personally, of being attacked by the malady, in its more intense degrees, was not so imminent.

In conclusion, I affirm, that the results of my treatment, all circumstances considered, were not only successful beyond precedent ; but such as to authorize me to consider an efficient method of cure for pestilential maladies as definitively established. But, whilst I do not wish, or cannot expect, that the truth of inferences so important, should be allowed to rest upon my bare affirmation, on the one hand ; so, I cannot permit, that its refutation should, on the other hand, be suffered to depend upon the assertion of the Turkish government, the Directors of the Pest House, their servants or agents, or indeed upon that of any human authority. Scientific is not of the nature of legal evidence. Were a hundred thousand infidels, and as many fanatics, to assert, or to swear the contrary, similar processes, conducted under similar circumstances, would not cease to produce similar results. But, if Turkey be not the proper theatre for bringing such matters to an issue, surely there are fit theatres to be found. And I do, in justice, expect, that, before any adverse decision be formed, I shall be furnished with the

opportunity, by a repetition of my experiments, under circumstances not so directly hostile to investigation, of shewing, that my allegations of success, in the treatment of this formidable disease, are conformable to the truth. Let those, who may choose to controvert them, be also, in fairness, required to produce *their* proofs from similar sources of personal knowledge. Induction of experiment, is not to be refuted, by the logic of misrepresentation, or numbers.

Whilst I was at Bucharest, a Greek of some rank and fortune wished me to prescribe for his wife, who had been for some years deranged, in consequence, as was believed, of loss of blood, during an accouchement. It was contrived that I should, as if by accident, see the lady at an evening party. But as, in a disease of such long standing, a cursory advice could have been of no service, nor probably a more methodical treatment, I declined prescribing for her. The husband of this lady, and others of the more wealthy inhabitants, were exceedingly desirous that I should settle in the town.

CHAP. XXXIX.

Departure from Bucharest—arrival at the Contumatz—our quarantine limited to thirty-six hours—dine at the Red Tower—arrive at Hermanstadt—hospitable reception by General Keinmayer—scene of a novel nature at the table d'hôte—Temeswar—Pest—visit Dr. Küttel—Presbourg—Flying-bridge—arrival at Vienna—visit Mr. Gordon, Secretary of Legation—Mr. de Hammer, Professor Frank, Count Harrach, Professor Prokalka—Departure from Vienna, and arrival at Paris—results of an interview with the Duke de Richlieu—arrival in England.

WE left Bucharest on Friday, the 10th of November, at ten o'clock, A. M.; and arrived on Sunday, the 12th, a little before midnight, at the Contumatz, or quarantine station, on the borders of Transylvania. We were shewn, with great formality, into an apartment of the best description, which the place afforded, and there locked up according to rule. There was a room for ourselves, and one for our servants. The apartment was good, but without furniture. There was a fire-place, a bench to sit on, and a broad platform at one side, in the manner of a kitchen-

dresser, which was to serve as a bedstead. With straw, cloaks, and great coats, we contrived to make beds, on which we slept as sound, as if they had been of down.

The Contumatz is a village, consisting of about twenty houses, and a Lazaretto for goods, situated in a small valley, so deeply immured in hills, that you can only see glimpses of the sun from above, as if you were looking at the heavens from the bottom of a brewer's vat. The village itself looks neat, and the houses are generally good, though small, being recently built. As travellers of every description, coming from Wallachia, are detained here, generally for eleven days, and frequently longer, but sometimes shorter, according to the existing apprehensions of pestilence, we were not surprised, on going to view the place in the morning, to observe several genteel equipages in this obscure valley.

We found that orders had been received, at the Contumatz, from General Kienmayer, Governor-General of Transylvania, and Colonel Schvind, of the Rhoten Thurn (Red Tower), in the neighbourhood, who has the immediate superintendence of the establishment, to limit the duration of our quarantine to three days, including those of our arrival and departure. Sunday being admitted as one day, because we arrived

before midnight, and the morning part of Tuesday being reckoned as another complete day, which, with the whole of Monday, made out the three days, we were permitted to depart a few minutes after twelve o'clock on Tuesday. Thus, by an exercise of peculiar indulgence, we suffered a detention of only thirty-six hours.

On Tuesday, the 14th, we dined with Colonel Schvind, and his family, at the Rhoten Thurn; and between five and six o'clock reached Hermandstadt. We immediately proceeded to pay our respects to General Kienmayer, and to thank him for the great indulgence, which he had been pleased to extend to us, by shortening so considerably the duration of our quarantine. He received us both cordially and courteously; and invited us to join his lady, and a party, who were passing the evening with them. We wished to excuse ourselves on account of our dress as travellers; but the honest soldier would not take a denial. Feeling the conviction, to which we had lately been strangers, that we were in civilized society, we passed the evening most pleasantly, alternately in conversation, and listening to songs, and music, in the best Hungarian and Transylvanian taste. Understanding that we did not propose to proceed on our journey, the General invited us to dine with him on the following day.

Wednesday, November 15. Having to return the Cronstadt waggon, which conveyed us from Bucharest, it was necessary to purchase a carriage for the rest of the journey, which detained us a day.

Upon our return from visiting General Kienmayer, a scene of a novel nature took place, in the evening, at the hotel, at which we lodged. Desirous of mixing with the society of the place, we requested to sup at the table d'hote, or public table, at which there were about twenty persons assembled, all officers of the Austrian army. The conversation happened at first to turn upon the Baron's father, the Internuncio, and upon German titles, which amused us, but in which we took no part. After some time, it took another direction; and a warm dispute arose respecting myself, some of the company asserting that I was living, and others that I was dead. A gentleman directly opposite to me declared that he had seen my death announced in the newspapers, and that he had no doubt of the fact. Hereupon, a young gentleman, who had seen me the preceding evening at General Kienmayer's, whispered to an officer near him that I was actually at the table; upon which an immediate appeal was made, for information, to Baron Sturmer. It would have been amusing to an indifferent

spectator, to have observed the various expressions of surprise, astonishment, doubt, and goodwill, which were manifested in the countenances of the persons present, upon hearing the confirmation of the fact by my fellow-traveller. They all stood up, as of one accord, and he who appeared to be the senior, and a colonel in the army, addressed me in a short speech, in the German language, thanking me in the name of his fellow-officers, and of humanity, for the services which I had been endeavouring to render to mankind. I requested Baron Sturmer to express to the colonel the great gratification I experienced at finding my feeble efforts so favourably appreciated, in a country, to which the calamities of pestilence were not unknown.

Upon our retiring, they all accompanied us to the door, with many expressions of kindness, and good wishes.

We left Hermanstadt on Thursday, the 16th of November, at four o'clock P. M. We had not proceeded many miles, when the carriage was overturned, by which the body was detached from the axle-tree; and we were obliged to walk the greatest part of the post. After much disagreeable travelling, and being repeatedly obliged to engage bullocks, in addition to our horses, to drag us out of the mud, we arrived at Tameswar, on Tuesday, the 21st of November, at two

o'clock in the morning. Having each passports as couriers, we were, in virtue of this privilege, allowed, by the corporal upon duty, to proceed directly to an hotel, without being previously sent, as is customary, to be examined at the main guard. Being informed, on the following day, that, for this deviation from strictness, the corporal had been ordered under arrest, we solicited, and obtained his release.

From Tameswar, we proceeded on our journey at four o'clock P. M. and on the 26th of November arrived at Pest, where we lodged at the hotel of the Seven Electors. I visited Dr. Küttel, my correspondent of the Seven Towers, who is a physician of considerable practice, and good reputation, in the town. He appeared unfeignedly rejoiced to see me ; and was eager to manifest such civility, as the shortness of my stay would permit. Having viewed the city of Pest, as well as the ancient capital of Hungary, Buda, on the opposite side of the Danube, we amused ourselves, in the evening, at a ridotto, which was held at our hotel, and at which the citizens danced waltzes in their boots.

On the 27th, at ten o'clock A. M. we left Pest. On crossing from the left, to the right bank of the Danube, the roads became much better. The weather was very severe. We went out of our way, re-crossing the river, to visit Presbourg,

the second city of Hungary, remarkable for its flying bridge, or, to speak more correctly, its immense passage-boat. This conveyance, or platform, is constantly passing from one side of the river to the other, and effects each passage in somewhat less than a quarter of an hour. It is frequently full, and sometimes incapable of containing all the passengers who offer themselves. It is fastened, by long ropes, to a chain of boats anchored at some distance above ; so, that, being impelled by oars and long poles, it swings slowly across, without being carried down by the stream. Whilst we were waiting at the water-side, there landed from it, at one time, sixteen carriages and waggons, forty horses, and upwards of a hundred foot passengers ; although it was by no means full. The weather being very cold, we were glad to remain in the carriage, whilst crossing.

We arrived at Vienna on the evening of the 29th of November. I remained at the hotel of the Archduke Charles, and Baron Sturmer proceeded to his own apartments.

November 30.—Waited upon the Hon. Robert Gordon, Secretary of Legation, and acting Ambassador, in the absence of Lord Stewart ; for whom I had an introduction from Mr. Frere. Having stated to him my design of soliciting the patronage of the Allied Sovereigns, for the purpose of enabling me to resume my

experiments on the plague, upon an enlarged and improved plan; and explained to him the grounds of my expectations of a successful issue, he appeared to enter cordially into my views; and promised to sound the Austrian government, respecting their disposition toward sanctioning and supporting such a measure. It was, unfortunately, an insuperable bar to any immediate progress in this plan, that the Congress of Sovereigns was no longer sitting. And I had the farther mortification to find, that Count Stakelberg, the Russian minister, for whom I had an introduction from Mr. D'Italinsky, the ambassador of that nation at Constantinople, was absent, with the Emperor of Austria, in Italy. I gave the letter to Mr. Gordon, who promised to forward it to Count Stakelberg.

Friday, December 1.—Called upon the celebrated orientalist, Mr. Joseph de Hammer, for whom I had an introduction from Mrs. Rich. We agreed to visit, in the evening, the philanthropic physician, Count Harrach. Dined at Mr. Gordon's, in company with Dr. De Carro, whom I had formerly known, and some English gentlemen. Mr. Gordon, who had been in Persia, informed me, that, of fifty thousand Russian soldiers, employed in Georgia, ten thousand die annually, the greatest number of the plague. The sick are brought out before the

door, and the physician walks round them, beyond the presumed *infectious* distance.

In the evening, I was conducted by Mr. de Hammer to the house of Count Harrach, where I met with Dr. Louis Frank, nephew of the celebrated professor of that name, who had been a physician in the French army, and served in the campaigns in Egypt, and Dr. Groman, who had been in Wallachia, during the epidemic of 1813, and had also visited Constantinople. We conversed for several hours on the subject of the plague, of which Dr. Frank, and Dr. Groman, had both had some experience. The latter denied having prescribed phosphorus in that malady, as was reported at Bucharest. Neither of them seemed to entertain any doubt respecting contagion.

Saturday, December 2.—Count Harrach was so obliging as to conduct me, in his own carriage, to visit the venerable and enlightened professor Frank, who was pleased to receive me with much cordiality. It was gratifying to me to find, that, after a lapse of fifteen years, he should have still recollected me. We had met, in the year 1800, together with Dr. Gall, and some other physicians of that city, in consultation, on the case of an English gentleman, who was then ill at Vienna. Count Harrach left us together. During two hours, that I remained in conversation with the

father of German medicine, I may truly say, that I was as much edified and entertained, as during any equal period of my life. He related to me several projects of his, for the improvement of science, and the regulation of the universities of Pavia and Vienna, some of which had not been carried into execution. But he spoke of their failure with perfect good humour, and in the true spirit of philosophy. Scarpa he mentioned as a man that had been originally oppressed, but was afterwards encouraged as a person of his distinguished merit ought to be. He gave an humorous and interesting history of Dr. Valli; who, in the course of his researches, had been reduced to such difficulties, as to be obliged to submit to assist apothecaries and barbers, in order to procure a dinner. Having, in the course of his peregrinations, found it convenient to enlist in the army, he was constrained, in order to procure his release, to become the amanuensis of a Custom-house-officer. The other parts of the history of this intrepid investigator, have, in substance, been related in Chapter xxxv.

A committee, of which Dr. Frank was a member, had, some years ago, been appointed, by the Austrian government, to draw up a set of regulations for quarantine. Upon the proposition being made to print them, Dr. F. objected to it as useless, until the general consent of the powers

of Europe, especially the maritime states, and the Ottoman government, should first be obtained. Baron Sturmer, the present Internuncio at Constantinople, who was also one of the committee, was of the same opinion; but thought that the consent of the Porte could not be obtained. Selim IV. the then Grand Seignior, was applied to. He professed personally to have no objection; but it was necessary to consult the Divan. Upon the proposition being submitted to the Turkish council, the Mufti got up in his place; and, declaring it to be contrary to every principle of the Mahommedan religion, announced his determination to oppose, with all his influence, so pernicious an innovation. Most fortunately for mankind, the measure was abandoned. Dr. Frank had previously experienced some difficulty, in procuring a copy of their quarantine regulations, from the Venetian government, with whom these institutions originated, and with whom it was usual, respecting all matters of state policy, to observe much mystery.

December 3.—Had a long conversation with Mr. Gordon, on the subject of my researches in the Levant. He seemed to be fully persuaded that the principal powers of Europe would be disposed to unite, in carrying into effect my plan, for farther prosecuting the investigation of the plague, provided the British government would

give it the initiative; and he promised to speak upon the subject, on the following day, to the gentleman, with whom he usually transacts official business, in the foreign department.

Wrote to the Frankfort Journal, contradicting the assertions, respecting my proceedings, which had appeared in that paper, of the 11th of November, under the head of Constantinople news. Prepared also similar contradictions for the Saltzburg Medical Journal, and Huffeland's. Glanced over Lernet's "*Dissertatio de Peste*," a work printed, but not published, of which a copy had been lent to me by Professor Frank. The author shews, that epidemic diseases have been frequent in their ravages, in different parts of Poland; and treats particularly of the pestilences, which afflicted that country, in 1798, and Odessa, in 1812.

December 4.—Mr. Langford Brooke, of Cheshire, who was on the eve of departing for England, having offered me a seat in his carriage to Paris, I left my own for sale at Vienna. We agreed to set out on Thursday. Upon returning to the hotel, I found the cards of Professor Frank and Mr. de Hammer. The latter left a copy of his last publication, on Eastern literature, which he requested me to convey to the Rev. Mr. Rencuard, of Sydney College, Cambridge.

December 5.—Accompanied Count Harrach, to visit Dr. Prokalka, professor of anatomy, in the university of Vienna. This ingenious gentleman shewed me his collection of anatomical preparations, which are very curious. The capillary vessels of each organ are injected in a stile of superior delicacy. *Plicæ Polonicæ*—*Mollities Ossium*—*Hydrocephali*—enlarged heads from polypus—Bones thickened, and bones wasted—various species of monstrous infants—form altogether a very interesting collection.

Dr. Louis Frank presented me with a copy of his work, entitled, “*Collection d’Opuscles de Medicine Pratique, &c.*”

December 6.—Mr. Gordon informed me, that he had spoken, according to his promise, upon the subject of my investigation ; that it would be mentioned to Count Metternich, and communicated of course to the Emperor ; and, that, seeing the frequency with which some parts of the Austrian dominions were afflicted with pestilence, scarcely a doubt could be entertained of its obtaining his countenance and support¹.

¹ Count Metternich, and all the foreign ministers, being absent with the Emperor, in Italy, no opportunity occurred, during my stay at Vienna, of obtaining any farther knowledge of the sentiments of the Austrian government, or the diplomatic body, on the subject of my investigation.

Visited Professor Frank at six o'clock in the evening, and remained with him till nine ; his conversation being incessantly both interesting and instructive. He favoured me with a historical sketch of his progress in life, from his quitting the university, his becoming physician to the Prince Archbishop of Spire, his publishing his work on Medical Police, his being first appointed Professor at Gottingen, and afterwards successively at Pavia, Vienna, Wilna, and St. Petersburg ; and finally, his refusal of the offer of Bonaparte, when at Vienna, to make him his consulting physician at Paris. The ex-Emperor, when in the plenitude of his power, condescended, in pursuance of his favourite policy, of concentrating talent in the capital of his dominions, to employ the most refined flattery, to induce this able physician, to choose the French metropolis for his residence. Having invited him to dine at Schoenbrun, he sent his own carriage back with him to Vienna, an honour, which, as a French General, who asked permission of Dr. F. to accompany him in the carriage, took occasion to inform him, he never conferred on any one, for whom he had not an extraordinary consideration. He requested him to visit General Lasnes, when dying ; enquired whether he thought he was treated properly by the French physicians ;

and sent him a fee of three hundred louis d'ors, and a gold snuff-box, set with diamonds. Dr. F. excused himself to Bonaparte, for declining his munificent offers, on the ground of his wishing only for retirement ; and of his increasing infirmities, which had obliged him to quit St. Petersburg, with all the attractions of the immediate protection of the Emperor Alexander¹. The conversations of Bonaparte, upon this occasion, as well as of his body physician, Corvisart, and other persons of his suite, as related by Dr. Frank, were truly characteristic.

Disgusted with the mal-administration of medical affairs at Vienna, sick of the climate of Russia, and panting for repose from the fatigue and turmoil of active life, Dr. Frank, some years ago, had determined upon retirement ; and chose, for his place of residence, Fribourg in the Brisgau. But he soon found that the vicissitude was too intense. The monotony of still life could not but be unsuitable to the condition of a mind, which had been so long, and so conspicuously occupied, on the busy theatre of the world. He found it necessary to his comfort to return to Vienna : where, declining the drudgery of general practice, he now only engages in medical con-

¹ Jean Pierre Frank, besides his Austrian appointments, is, *Conseiller d'Etat actuel et premier Médecin de L.L. M.M. Imperiales de Russie*,

sultations. His nephew, Dr. Louis Frank, resides with him ; and is most solicitous, as he ought to be, in attentions to his illustrious relative. His son is a medical professor, in the university of Wilna, in Poland.

I am not apprehensive that any reader will be so fastidious as to consider this sketch, which I could not refuse myself the gratification to insert, of a truly valuable character, and an able physician, as being here misplaced. In talents, person, dignity of mien, and benevolent aspect, Dr. Frank strongly reminded me of another able and distinguished medical character, for whom, in common with all who knew him, I entertained the highest respect, Dr. James Anderson, late Physician-General at Madras. Dr. Frank's medical works are too well known to require enumeration.

December 7.—Took an early dinner with Mr. Popp, the banker, with whom I left some commissions to execute. Observing, in his antichamber, the picture of a very handsome man, in an eastern costume, he informed me it was that of a friend of his, an Armenian, whose name I have forgotten, who had amassed immense wealth at Constantinople ; and, being apprehensive that he might lose both his fortune and his head, had withdrawn himself from the Turkish territory. He is said to have it in contemplation, to employ his money, in building a town, upon

that part of the Russian territory, which is situated on the banks of the Danube, at its junction with the Pruth.

After dinner we set out on our journey, and arrived at Paris on the 19th or 20th of December. The next day, the barriers being shut, on account of Lavalette's escape from prison, it was with great difficulty that Mr. Brooke, who was under the necessity of proceeding immediately for England, could get permission to pass through, upon his consenting to take a police-officer in his carriage to the end of the first stage. I remained a few days longer, for the purpose of procuring an interview with the Duke de Richlieu, on the subject of my investigation of the plague.

On Wednesday, the 3d of January, 1816, the Duke de Richlieu, although extremely occupied with affairs of state of the most urgent importance, did not consider the subject of my researches undeserving of some portion of his immediate attention, and was pleased to grant me a long audience. We conversed in English, his Grace speaking that language more fluently than I do the French. It was not simply in his actual public character of Secretary of State, that I was anxious to procure the Duke de Richlieu's countenance and support for my investigation, although the concurrence of France could not but be of

essential consequence, both to that country, and to the world in general, on account of the quarantine system established, on so large a scale, at Marseilles ; but as an individual, who had had opportunities, during his government of Odessa, by a close inspection, and an active personal superintendence, of becoming intimately acquainted with the calamities of pestilence ; and whose sanction would consequently be of the greatest weight. He appeared to be well disposed toward my plan ; and professed himself willing, as a member of the French government, to contribute to the furtherance of any measures toward the completion of my researches, to which the British government might give origin. He regretted the impossibility, owing to my approaching departure, and his want of leisure, of entering more fully into the subject at that period ; but hoped that I should soon have occasion to revisit Paris, in which case he would communicate to me documents, respecting the great plague at Odessa, in 1812, upon which he could not then conveniently lay his hands.

My delay having no other object, I set off that evening, and, on the 7th of January, 1816, arrived at my house at Greenwich.

It had been my intention to annex to this part of the work, a sketch of the plan, which, upon my arrival in England, I thought proper to pre-

sent to the British government, respecting the application of the results of my researches to general use. But, as the original propositions have since undergone several modifications, or rather assumed a variety of new shapes, and as the matter is still pending, I conceive it will be better to defer any statement of the proceedings, which have taken place, until they be brought to a final issue ; the more especially as the validity of my discoveries, and the policy of acting upon them, are subjects of very distinct consideration,

PART IV.

OF EPIDEMIC AND PESTILENTIAL DISEASES¹.

“ But now sciences are delivered as to be believed and accepted, not to be examined and farther discovered: and the succession is between master and disciple, and not between inventor and continuer, or advancer; and therefore sciences stand at a stay, and have done for many ages.”

FIL. LABYR.

CHAP. XL.

OF THE CAUSES OF THE RETARDATION OF MEDICAL IMPROVEMENT.

“ It is unnecessary for the College to explain to their Lordships the uncertain nature of all medical evidence; but it is proper that their Lordships should be informed, that the history of Physic presents numerous instances of recorded

¹ The propositions, respecting epidemic diseases, which are here only stated, will be found explained, in reference to diseases in general, in a work shortly to be published, under the title of “ Elements of Medicine.” I am obliged, in order to avoid repetitions, to make this reference by anticipation.

facts, which, after having obtained credit at certain periods of time, have, by subsequent investigation and enquiry, fallen into disrepute, or have been disproved." *Report of the College of Physicians, to the Lords of the Privy Council, Nov. 15th, 1815.*

IN the improvements, which have been incessantly taking place, in almost every branch of science, in modern times, medicine, strictly so called, perhaps alone can be said to have scarcely participated. This will be found to be true, not only with respect to doctrines, but to practice. But it has not been the lot of this important department of knowledge to remain merely stationary : for, taken as a whole, it has manifestly undergone an unparalleled retrogradation. Thus, in respect to the cause of those diseases, which occasion the greatest sum of misery and mortality to mankind, we find the world farther removed from truth and nature, than they were upwards of two thousand years ago ; and, in respect to the practice, that the treatment of the ancient physicians, if seldom positively beneficial, was at least much less efficiently mischievous. If cures were not often performed, which might not have been equally effected by the usual and ordinary operation of the surrounding elements, the records, which have reached us, deficient as they are, will still justify the conclusion, that, in the most

remote ages, disease and death were not so frequently produced, by positive abstraction and depletion, as in the present otherwise more enlightened times. The extravagant pitch of wanton and murderous excess, to which, especially of late years, the imitation of the most irrational and barbarian practice, in question, has been carried, in several countries, whilst it reflects indelible disgrace upon the medical intellect of the age, affords an extraordinary, and perhaps singular example, in modern days, of the degradation of science, by an inexplicable spirit of fanaticism. The preposterous conclusion would seem to have been formed, that, in sickness, the living body can have no other tendency than to produce all its functions in excess; as well as that this tendency is invigorated in the precise proportion of the intensity of existing disease. For the extraordinary duration of this uncommon perversion of reasoning, there must, doubtless, have been an uncommon concurrence of causes: and, as these effects are more injurious, in the diseases which form the subject of my present enquiry, in proportion to their superior importance, than in other maladies, it is fitting that I should here investigate the nature of their causes.

These may be resolved, generally, into the hitherto uncertain nature of all medical evidence.

How much this uncertainty has prevailed, if it were not otherwise notorious, would be placed beyond all doubt, by the declaration, which I have chosen for the motto of this chapter, from an authority which cannot be suspected of wishing to disparage the art. Two questions will here immediately arise : is the evidence applicable to the affairs of medicine, in its very nature, uncertain ? Or have the results of the evidence, hitherto employed, been uncertain, only because it has been of an improper kind ?

Even if we were not, in fact, to find the evidence which may be applied to medicine, as certain in its nature, as that which is found to be appropriate in other branches of science, we should not be justified in concluding, that it were otherwise, since it must be impossible that any department of knowledge should be so unfavourably distinguished by nature, as to be left incapable of proof.

What, then, appears to be the real state of the case ? Beside the causes, that have contributed to retard the advancement of science in general, circumstances of a peculiar nature have concurred to impede the progress of medical improvement. Half a century before the introduction of the true method of philosophising, by Lord Bacon, the doctrine of contagion, in epidemic diseases, had already established itself in Europe ; and laid

the foundation, not simply of a stagnation of medical knowledge, but of that positive retrogradation, which has even continued progressive to our own times. The spiritual influence, by which this absurd doctrine was upheld, would not sanction any other species of evidence than those of tradition and testimony, which it could always render propitious to its views: and it is upon such evidence, that even the most modern of our writers, upon epidemic diseases, have vainly attempted to establish, on each side of the question of contagion, what they have improperly denominated "*proofs*."

As the employment of tradition and testimony, for induction of experiment, has formed one; so the difference between living and dead matter, as objects of investigation, has formed another powerful cause, of the retardation of medical improvement. The phænomena of life, particularly of the functions in a state of disease, are not only so different in different persons, but in the same person at different times, as to have favoured an opinion, that they are not subject, like other parts of nature, to any fixed laws, cognisable to the senses. Although, under the ordinary circumstances of health, the effects of food, air, exercise, and the passions, were perceived to be almost uniformly similar, yet the great diversity of the phænomena, indicating disease, has been

supposed to preclude the possibility of any immutable relation between them and the circumstances, by which they were produced. Self-complacency came readily to the aid of this delusion : and man, in comparing himself with other objects, found some consolation for his ignorance of his own nature, in the supposition, that it is of so exalted a kind, as to be capable of being known only to the Author of his being.

This appears to be a principal reason, why, in medicine, so few, if any experiments have been made, with a view to ascertain certain and determinate results. There is not, perhaps, an instance, on the records of ancient or modern times, of a faithful communication to the public, of any satisfactory conclusions deduced from accurate experiments, or of the processes by which they were alledged to be obtained. Alexander, indeed, gives us the history of two cases, with his method of proceeding in a tertian and schirrus of the spleen ; “ which,” says Friend †, “ except what we read in Hippocrates and Galen, and those not so particularly stated, are the only examples of this kind in antiquity.” But, neither these, nor any other that I know, have afforded any thing like precise or determinate results : consequently they have not pro-

† Hist. of Med., vol. i. p. 84.

duced the smallest benefit ; but rather, I think, the reverse.

Inaccuracy, and bad faith, in the reports of patients, are also most extensive sources of fallacy, in regard to medical conclusions. The reports of patients are usually so extremely vague, contradictory, and uncertain, that, in believing them, man has almost appeared, even to good observers, to constitute an exception to the usual uniformity of nature, in the relation of cause and effect. For this reason, physicians, without being less philosophers than other men, have, in the exercise of their profession, been more subject to delusion. Every practitioner of medicine will easily recollect a great many instances, in which he has, to his knowledge, been grossly imposed upon, by his patients ; and, he may, from thence, justly infer, that, in a great many more instances, he must, without his knowledge, have been similarly deceived.

Thus, results, dissimilar, or opposite, are represented, with equal confidence, as having been obtained by the application of the same remedies, in the same diseases : and this contrariety of inference, if we suppose equal good faith to exist in all the parties, can only be accounted for by a disagreement in the reports of the patients, or by a difference in the mode of administering the same powers, or both.

This reasoning is of general application. From ignorance that all the phænomena of life are the result of the combined action of external agents upon the living body, and of the appropriate set of phænomena which depend upon the action of each, have arisen those eternal disputes, respecting the virtues of particular remedies, which have disgraced the medical profession in all ages ; and an almost childish eagerness in search of new ones.

Hence, we shall find the extravagant praises, and condemnations, alternately bestowed upon each article of the *Materia Medica*, to be almost equally without foundation. The want of principles, to guide us to a sure and fixed practice, has occasioned that frequent transition from one fashionable mode of treatment to another, which has rendered the profession of medicine so frequently ridiculous, even in the eyes of the vulgar.

The principal step in every science, is the establishment of some fundamental principles, to which we may constantly recur, as monitors, to direct us to right, or to recall us from wrong tracts of investigation : and until these be acknowledged in medicine, it must remain, as it has hitherto been generally considered, in the mean and degraded condition of a conjectural art. But a certain set of fundamental principles

being once agreed upon by common consent, any difference which could arise in their application, or detail, would be scarcely perceptible. All the sources of fallacy and dispute, respecting the virtues of particular remedies, or the nature of particular diseases, would vanish: and, medicine, instead of being the sport alike of the vulgar, and of the learned, assuming the lustre which, of right, belongs to it, would take an elevated station among the most dignified, and the most useful of the sciences. Thrown into action, it would constitute an art, surpassing the art of medicine, in its conjectural state, as much as the modern surpasses the ancient art of navigation.

The appropriate effects of each agent being ascertained and established, it would be impossible for patients, or their attendants, to practice those arts of delusion on the physician, which they now so frequently employ. If I were, for instance, to prescribe a remedy, of which I perfectly knew the usual and constant effects, but these effects were not produced, what ought I to conclude? The laws of nature, or the relation of cause and effect, can never vary. The same agent cannot, therefore, act differently in different persons, if placed under similar circumstances. I must consequently conclude, either that there is a deception in the force of the agent

directed to be applied, or a fallacy in the report of the patient, or that I have formed an erroneous judgment respecting the circumstances under which he is placed. With principles as a guide, it will be easy to ascertain wherein the error lies.

To the uniformity of relation between cause and effect, which is here presumed to exist in medicine, it will, perhaps, be objected, that, as it is very difficult to ascertain the degree of similarity of circumstances, in which living bodies are placed, the physician, supposing him to have a general knowledge of the principles upon which medicines act, may frequently form a mistaken judgment. But, in answer to this, it may be observed, that, as in respect to the action of agents, the circumstances under which living bodies are placed, can only differ in degree: so, in disease, by proportioning the force of the agents to the degree of disease to be removed, living bodies can always be placed under similar circumstances, excepting in those degrees of disease to which no remedy can reach.

I am, however, very far from thinking, that, in the application of any science, perfect precision can be attained. No two houses, no two ships, can be built so entirely alike, as not to be distinguishable. Even in the phænomena of nature, which most resemble each other, there are

always shades of difference; in so much that no two series of phænomena were ever, perhaps, precisely the same. No two stones, apparently of the same size, weight, and form, ever fell to the ground, from the same height, in a manner entirely similar. In living animals, no two individuals, of the same species, ever resembled each other so completely, as not to be distinguishable by other individuals of their kind. No two cases of disease, which have obtained the same name, were ever exactly alike in all their symptoms. But, in those successions of phænomena, of which the knowledge constitutes the science, there exists a similitude sufficiently close, by which to form rules to guide the practice of the art. And medicine, if its proofs be allowed to depend upon the evidence proper to the subject, we have no reason to presume less capable of being founded upon certain rules, than any of the other sciences.

Another cause of the retardation of medical improvement, is the application of a false analogy to the explication of the laws of living bodies. Medicine, in respect to its subject, labours under very great disadvantages, when compared to the other sciences. With inanimate objects, experiments are fearlessly made and repeated, and the results attributed to them easily refuted or confirmed. But with the living, and more espe-

cially the human body, attempts to ascertain unusual results, by unusual means, are apt to be considered as of the nature of sacrilege; and, their repetition, if not interdicted by authority, to be repressed, by the dread of mischief, or of censure. Thus the apparent difficulty of obtaining precise results, by direct experiments on the living body, has led to the application of false analogies to medicine, derived from the sciences, whose objects consist of inanimate matter. Such has been the source of the humoral pathology; and of all the hypotheses, whether chymical, mechanical, hydraulical, or corpuscularian, which have been successively erected into systems, and have flourished, and passed away, with the influence of their respective authors.

All modes of false reasoning, although common to all times, may be said to have become more general, since what has been called the revival of learning. Upon the introduction of better modes of philosophising, it might have been expected, that, medicine, keeping pace with the other sciences, would have assumed a regular and consistent progression. But this was, by no means, the case: and we have even to regret some very recent attempts to explain the operation of remedies, upon living bodies, by their properties, as chemical agents. It is a fact, which should be constantly held in view, by the physician, that, the

laws of life are of a peculiar kind, and have no more connection with the laws of chemistry, than they have with the laws of mechanics. However these sciences may, in the result of their progress, point toward some common principle, it appears necessary, at least in the present state of our knowledge, to consider them as distinct and unconnected. Every step in such analogy, as leading to a labyrinth of error, should be carefully avoided.

It seems, indeed, to be a palpable fact, not only that the medical effects of agents do not, by any means, correspond with their chemical properties ; but, that, agents, possessing chemical properties of a different, or even of an opposite nature, may, as exciting powers, produce similar effects. Sub-muriate of mercury, supertartrate of potass, digitalis, and squills, have all been found beneficial in dropsy ; nitric acid, and caustic vegetable alkali, have been used with success in the treatment of the same local affections ; and, agents of the most various chemical properties, are known to produce vomiting, purging, sweating, and an increased discharge of all the secretions. We are, then, warranted in concluding, that the choice of agents, for the cure of diseases, does not depend upon any of their known chemical affinities ; but,

upon principles, which are peculiar to their operation as exciting powers.

Were it even true, as has been alledged, that the power with which agents act upon the living body, is proportionate to the quantity of oxygene, or any other principle, they contain; it would not follow that they act by chemical affinity. Oxygene would, in that case, indeed, occupy a principal rank among the exciting powers; or would perhaps be the most general of them all. But the mode of operation of those agents, of which it forms a component part, in occasioning, preventing, and curing diseases, would still be the same; and consequently the rules respecting their employment as remedies.

The only case, in which, it would appear, we can bring chemical affinity to our assistance, in the living body, is when an agent of sufficient power to act as a poison, has been received into the stomach. The immediate introduction, before it has already acted, of a body, capable of neutralising the poison, or of rendering it inactive, will prevent the bad consequences, which would otherwise result from its operation; but the full effect of the noxious substance having taken place, no further aid can be derived from chemical affinity; and, if disorganisation should

not yet be so extensive as not to admit of a cure, it is only to be effected by the application of powers capable of restoring the excitement, according to the known laws, which are peculiar to living matter. But it is seldom, even in this view, that chemical affinity can be efficiently employed : for, the operation of poisons being quick in proportion to their intensity, disorganisation is, for the most part, so speedily produced, as, even when the poison is known, rarely to leave time for assistance. When two agents, meeting in the stomach, or alimentary canal, by chemical affinity, produce a third, which is different from either, the action of this new body on the stomach, or intestines, and through them on the system at large, will be still nothing else than that of an exciting power.

If the operation of agents, as exciting powers, depended upon their chemical properties, a knowledge of these would determine their efficacy as remedies ; which is not found to be the case.

BOOK IX.

OF THE NAMES, PHÆNOMENA, AND HISTORY OF EPIDEMIC DISEASES.

CHAP. XLI.

“ Science has much to deplore from the multiplicity of diseases. It is as repugnant to truth in medicine, as polytheism is to truth in religion. The physician who considers every different affection of the different systems in the body, or every affection of different parts of the same system, as distinct diseases, when they arise from the same cause, resembles the Indian or African savage, who considers water, dew, ice, frost and snow, as distinct essences: while the physician, who considers the morbid affections of every part of the body (however diversified they may be, in their form or degrees) as derived from one cause, resembles the philosopher, who considers dew, ice, frost and snow, as different modifications of water, and as derived simply from the absence of heat.” *Rush's Account of the Bilious Remitting Yellow Fever, &c.* p. 29.

The arrangements of nosology unfounded—names of epidemic diseases—their symptoms—description of the plague by Fra Louigi di Pavià, of Smyrna, communicated by Mr. Thomson, a Member of the Levant Company.

THE first step toward true knowledge, in any department, is, undoubtedly, to unlearn that

which is false. It is difficult to say, to what part of medicine, strictly so called, this remark will not apply. But, of all the false knowledge, which exists, in the various branches of this science, that, which has, in modern times, been most systematised, and rendered most pernicious, is what relates to the classification of diseases, under the title of nosology. From the days of Hippocrates, to the revival of learning, the doctrines which prevailed, where any have existed, were those of the ancient physicians. If the progress was small in actual knowledge, it was also small in sophistication. But, after the revival of learning, the translation of the Council of Trent, the invention of the art of printing, medical doctrines the most false and pernicious, were promulgated by the highest human authority. These absurd and mischievous dogmas were taught by universities, colleges, and schools; and farther propagated by impressions indefinitely multiplied by means of the press; by which it happened, that the mind of the medical student, before his entrance into life, was sophisticated, generally beyond the possibility of future emancipation.

At a period still later, was reared the extraordinary fabric of methodical nosology. By the labours of successive systematics, the multiplication and classification of diseases, which, according to

nature, are but one, was carried to such a degree of refinement, that nothing more was left for human ingenuity to invent: and the enquiring mind, disgusted and bewildered, found itself compelled to attempt to return to first principles.

Thus, our first and most indispensable proceeding, in respect to the classification of diseases, is, to unlearn all, which is not to be found in the arrangements of nature, of what we have been taught at the universities, colleges, or schools of physic; and, afterwards, to endeavour to ascertain, in what respects diseases really do differ; and of what subdivisions, or classifications, they admit, according to truth, and for purposes of practical utility.

I shall not here enter into any exposition of the erroneousness of the existing systems of nosology; but proceed, at once, to give what I conceive to be the true view of the subject, by stating what appear to me to be the distinctions, existing in nature, in respect to the morbid affections of the living body.

By investigating the subject *de novo*, and as if no nosological doctrines had ever been known, we find the results to be, that diseases do not differ from each other, in any other manner, than in extent, situation, and degree; and, that, consequently, there cannot be, in respect to their

nature, any specific ground of distinction, between maladies.

What, then, are the grounds of distinction, established by nature, between the morbid affections incidental to the living body? They are to be found, if I mistake not, in the difference of their remote causes, which indicates a correspondent difference in their means of prevention. According to this principle, diseases may be divided into three classes: 1. Those which depend upon the air, and are called epidemic or pestilential; 2. Those which depend upon a specific virus, capable of propagating itself from person to person, and are called contagious; and 3. Those which do not depend upon either of these causes, and may be called sporadic.

These, again, have their subdivisions, according to distinguishing characteristics, and especially according to the organs, which they principally affect. This explanation is sufficient for my present purpose. The general doctrines are, as I have said, discussed elsewhere. In the meantime, I proceed, according to these principles, to examine the first class, which constitutes the subject of our immediate enquiry.

The term "epidemic," has been employed without any reference to their cause, to denote diseases, which affect a great number of persons at the same time. This can only happen of mala-

dies, which are occasioned by a cause of general operation, as the properties, or vicissitudes of the atmosphere. Hence, although a disease arising from this cause, may, in its lowest degrees, be occasionally confined to one, or a few individuals, as in respect to catarrh, cynanche, &c., and might, then, in strictness be considered sporadic; yet, upon the whole, the term epidemic, may, at least for the present, or until a more appropriate one can be determined upon, by common consent, be advantageously retained, to denote the diseases that depend upon the atmosphere; comprehending all the intermediate degrees of affection, from the slightest catarrh, to the most destructive pestilence. The species of pestilence, incidental to the Levant, and to all other countries similarly situated, in respect to latitude, cultivation, and soil, &c. has been denominated "plague." Epidemic diseases, pestilential diseases, and plague, although these words possess, strictly speaking, some difference of meaning, may, for the most part, be used as synonymous terms: and plague, which comprehends in itself, every variety, and degree, of this class of maladies, may be aptly regarded as the representative of the whole.

The names derived from any particular symptom, as the colour of the skin, as in yellow fever, scarlet fever, &c. or the heat of the body, as in

typhus ; or the nature of the matter discharged, as bilious ; or the supposed seat of the malady, as *fièvre gastrique* ; or the situations or countries, in which it usually arises, as jail, hospital, or ship, Bengal, Batavia, Bentoolen, Bulam, Gibraltar, fever, &c. ; or the persons whom it attacks, as *fièvre matelotte*, are, in a scientific view, entirely improper, as having no reference either to the nature, or the cause of the disease ; as constituting distinctions, that are of no practical use. The colour of the skin, or the other phænomena or symptoms ; the situations in which diseases arise ; or the countries to which they are incidental, do not necessarily infer a difference either in their nature, or their cause. The nature of disease is always the same, one affection differing from another only in extent, situation, and degree ; and the cause of the epidemic affections, which have thus obtained so many different appellations, is the same, differently modified in different countries, and situations ; applied, in less or more intensity ; with less, or more, duration ; and to organs previously possessing various degrees of excitement, or vigor : and thus producing phænomena, or symptoms, of infinite diversity. Names, founded upon such diversity, might, and, if it were a proper foundation for a nomenclature, ought, in consistency, to be extended to each series, or cluster, of symptoms ;

and, thus, our nosological systems, instead of being shortened and simplified, would be infinitely complicated and extended.

Upon such grounds, fevers might be divided into a thousand, nay, a hundred thousand, *genera*; for the combinations of symptoms, which denote the various degrees of deficient excitement of the circulating system, and of the other organs, which are, in particular cases, liable to be, at the same time, affected, would admit of an endless variety of appellations. Thus the *Causus*, or *Febris Ardens*, of the ancient Physicians; the *Plague* of the *Levant*; the *Typhus* of modern systematics; the *Yellow Fever* of *America* and the *West Indies*, called also *La Maladie de Siam*, and *La Fiebre Matelotte*, by the French, and *Vomito Prieto* (*Black Vomit*) by the Spaniards; the *Putrid Synochus* of *Bontius*; the *Febris Ardens Biliosa* of *Towne*; the *Malignant Fever* of *Warren*; the *Putrid Bilious Fever* of *Hillary*; the *Endemial Putrid Fever* of *Lind*; the *Endemial Causus* of *Moseley*; the *Typhus Icterodes* of *Sauvages*, *Cullen* and *Selle*; the *malignant pestilential Fever* of *Chisholm*; the *Febris Indiæ Occidentalis Maligna Flava* of *Mackittrick*; the *Febris Flava Americæ* of *Lining*, *Cary* and *Clark*; the *Bilious Remitting Yellow Fever* of *Rush*; the *Putrid Bilious Remitting Fever* of *Balfour*; the *Bulam Fever* of *Pym*; the *Medi-*

terreanean Fever of Burnet ; the Calentura of the Spanish writers ; the Hill Fever of the coast of Coromandel, and the Jungle Fever of the northern parts of Hindostan :—These, and many others, affording a curious variety of nomenclature, are all names to designate different modifications and degrees of the same malady. Some of them, as I have said, have been derived from the distinguishing symptoms, the supposed nature, the type, or the severity, of the disease ; some from the country, in which it has appeared, or from which it has been supposed to be imported ; some from the description of persons principally affected by it ; and, finally, some from several of these circumstances combined.

But, as morbid affections can only differ from each other, in extent, situation, and degree ; so, the diseases, which have obtained so great a variety of names, are all precisely of the same nature : and, if even hitherto unknown remote causes of disease should arise, they could not possibly produce any new maladies. Their morbid effects could be no other, than a diminished excitement, of an organ, or organs, or some parts of an organ, in various combinations, proportions, and degrees ; differing, in no respect, in their nature, from the diseases already known to be incidental to these organs. Even a new contagious virus could produce no other morbid effect

than a diminished excitement of those organs, upon which it might preferably operate. But it is easy to perceive how the possible combinations, proportions, and degrees, of morbid affection, of organs, may produce symptoms, or phænomena, that are of unusual occurrence; which may, according to the erroneous classifications of nosology, give rise to the delusive idea of new diseases. Hence, the idle disputes, concerning the nature, and origin, of the fevers, which have recently ravaged the fortress of Gibraltar, and the towns of Spain; as if they were not the very same pestilences, by which these countries have been afflicted, at the same season, for centuries, or rather from the origin of societies.

Having made these objections to the existing nomenclature of diseases, I shall still think it right, in order to avoid the confusion incidental to the hasty introduction of new names, to continue, in speaking of epidemic maladies, to employ the appellations, by which they are most generally designated, or known; and, when it is not necessary to distinguish particular modifications, generally to use the term "*plague*," as the representative of this whole class of maladies.

If we consider, that, in diseases occasioned by the air, the whole mass of blood must be primarily affected, it will appear obvious, why, in these maladies, not only all the organs should be

diseased, but there should be so great a diversity in the degrees, in which they are affected, and consequently in the phænomena of epidemics, in different individuals, in the same country, and in the same season. It is also obvious, that, as, to enumerate the symptoms of even one pestilence, would be to state almost every variety of appearance, which each organ of the living body can assume, in its diseased state; such minuteness would be any where superfluous, but, in a work, the object of which is of a general nature, and limited to an elucidation of principles, wholly misplaced.

It will therefore be sufficient, for my purpose, to refer, for a description of symptoms, to the authors, who have written upon the epidemics, which they have seen, or treated: and the more especially, as symptoms, being objects of the external senses, admit of an accurate description, and of a general agreement; or rather scarcely admit of a dispute. All that I shall think it necessary here to state, upon this part of my subject, will consist of such matters as have come immediately within my own personal observation, or have been communicated to me, by others, from credible or authentic sources. The former I have already stated in the narrative of my proceedings in the Levant, and in my Report to Sir Robert Liston. And, of the latter, I beg here

to communicate some observations of a highly respectable person, who had, for thirty years, assiduously attended plague patients; and, in as far as relate to the symptoms and prognostics, may be presumed to have acquired a competent knowledge of the malady. I mean Fra. Louigi di Pavia, prior of the Hospital of San Antonia, at Smyrna. His remarks are here inserted, literally, and without alteration, as they were communicated to me by the favour of Mr. Thomson, a member of the Levant Company; which would have authorised me to consider them as genuine, did they not bear such intrinsic evidence of authenticity.

“ The symptoms to know the plague, and the different tumours and buboes which make their appearance on the first attack of that dreadful disorder.

“ The plague, of all the distempers which afflict man, and fill up the measure of human calamity, is perhaps the most cruel and deadly. It is as various as the complexions and constitutions of those unfortunate persons whom it attacks. In some strong and robust constitutions, the signs of the disorder are evident and visible, in others they are less so. People of a slender habit of body attacked, throw out various eruptions, which, for some time, make the disorder

doubtful; nevertheless, I may venture to state, after thirty years attendance upon people of various nations and employments in the sick hospital at Smyrna, that the following observations are nearly correct.

“ Either in a strong or weak person the surest symptoms of the plague are, the eyes dusky and turbid, fixed and sparkling; the tongue forked, of a whitish colour, inclining to yellow, with the extremity red, which branches out into a number of small ramifications exceedingly inflamed; a violent pain of the head, with frequent vomitings, occasioned by most violent heavings of the stomach, as if turned upside down, accompanied with frequently biting the lips, and finally, with a total loss of strength, and languor, which pervades every member of the body to such a degree, that the miserable patient, however courageous he may be, cannot help expressing the greatest disgust for every thing offered to him and abandoning himself to the deepest melancholy. It is necessary, however, to remark, that all these symptoms do not usually shew themselves at the same time, nor on the same person, but only a few of them. Debility and languor throughout every part of the frame, however, universally follow an attack of the plague, whether violent, or modified by unknown causes.

“ This terrible disorder is ever accompanied

with some degree of fever, which will ever be greater or weaker in proportion to the violence of the attack. If the pulse is strong, and, as it were, takes sudden leaps, you may rest assured the attack is strong, and a speedy dissolution may be looked for. The infected are often subject to hemorrhage at the nose. If this is violent and continued, it indicates fatal consequences; if gentle, and of short continuance, you have great reason to expect a safe recovery. In the beginning of an eruption, the eye on the side it began upon very soon becomes smaller than the other, so much so, that it is easily observed by any person present.

“ In general the plague begins with cold fits, which are succeeded by warm ones as in the ague, sometimes it begins with sudden startings, and an involuntary action of the body, often complicated with some other disorder. After the eruption has taken place, if the patient is tortured with spasmodic pains in the abdomen, it is a sure sign of an approaching dissolution; these can seldom be removed, being the forerunners of a mortification in that region.

“ In proportion as the eyes turn clearer, and the vision more perfect, the hopes of recovery are stronger.

“ In this terrible malady, anxiety, languor,

and melancholy, indicate internal convulsions, nervous attacks and death at hand.

“ When the tumours are long, hard, and well rooted, they are favourable symptoms, and, on the contrary, if round, soft, and superficial, they announce death,

“ Sighing, if short and mild, is only a sign of weakness; if profound and deep, the sign of death. The fever, which accompanies the plague, is commonly slight: if it is severe, it ends in death.

“ After nine, or eleven day's illness, great hopes may be entertained of a cure (recovery) if the infected person does not labour under a *thanatolitta*, or *mavrotighano*. The delirium to which those afflicted with the plague are subject, if of short duration, is a good sign, and followed by a fresh irruption, if long and violent, the worst sign, and generally brings a speedy dissolution. The true plague makes its eruptions under the arm, or in the groin, or near the ears; this is called the mother plague; all other tumours, which appear upon the body of an infected person, are called *carbuncles*, and are of five kinds, named:—

“ *L'antrace* is a black bubo, which spreads out some red borders, inclining to purple; the worst sign of all, and sure death.

“ Evloito is hard, and round, desirable in every case.

“ Ambelocladhi is of a good shape, regular, and assures you of the patient's life.

“ Thanatolitta is, at the root, very hard, rather small, and dun colour, and like a hard pea in roundness, and if it does not suppurate by the eruption of some new humour of good quality, that is, by evloito, or ambelocladhi, it extends itself, turns black, and is deadly.

“ Mavrotighano is dry, rough, and black, which dilates, and turns very large, having only seen it cured twice in thirty years, which I have attended those afflicted with the plague, caused by a fresh eruption of one of the forementioned good humours.

“ Ambelocladhi continues many days, and produces a great number of worms, which devour all that dry flesh which composes it, and makes it appear full of holes, like a rough sponge. The best manner to destroy these vermin is ointment applied three or four times a day, together with parsley leaves well dried, and beat in a mortar, sprinkled with spirits of wine.

“ A long and violent diarrhœa, which can seldom be stopt, is sure death.

“ So far I can state from long experience, respecting this dreadful disorder; not being a

medical man, it is impossible for me to describe the symptoms, or the disorder technically. It, perhaps, may not be necessary, *never having seen any cure performed according to the rules of art.*"

This, although short, is to me the most satisfactory description of the symptoms of the plague, that I have ever perused. It appears, that, at Smyrna, they distinguish, beside the mother plague, five other modifications, denoting, by their peculiar phænomena, as many different degrees of intensity and danger. To observe these phænomena, and from them to prognosticate the event, will necessarily form the principal employment of those who make it their business to superintend plague patients, whilst ignorance prevails respecting an efficient method of cure. But, the principles, upon which the cure of the disease is founded, being established, the doctrines of prognostics, and of critical days, as well as many other inanities in medicine, for which there will be no longer occasion, will cease to be regarded, in the same degree.

CHAP. XLII.

OF THE HISTORY OF EPIDEMIC DISEASES.

As the air must, at all times, and in all places, have been liable to become occasionally noxious ; and as there is no portion of the earth, however salubrious in itself, to which a pestilential atmosphere may not sometimes extend ; so, if even history had not placed the fact beyond a doubt, we should not have been justified in not concluding, that epidemic and pestilential diseases have been coeval with societies.

The history of every country, of which authentic records exist, teaches us, that, in all of them, and at all periods, epidemic diseases have been liable to prevail, at uncertain intervals, but at determinate seasons, as they continue to occur at the present day.

Accordingly, we find, that, in Egypt, in Greece, in the Roman empire, and in all other inhabited portions of the globe, the pestilences, which were wont to occur, several thousand years ago, have been of the very same nature, distin-

guished by similar phænomena, happening at intervals of equal uncertainty, and commencing and ceasing at seasons equally determinate, as those epidemic affections, of which we ourselves have been the witnesses.

These are the diseases, which principally and deservedly occupied the attention of Hippocrates : and the phænomena, which he describes, as distinguishing them, correspond both with those which distinguish the modern plague of the Levant, and what has been called yellow fever, in other countries¹. All these, and every other modification, of febrile affection, occasioned by the air, it was the practice of this great man to include in his simple, but comprehensive title of epidemics, so superior to the confused, artificial, misleading, and multifarious nomenclature of modern times.

As it does not admit of a question, that the epidemics of Greece occur, now, at intervals equally uncertain, and at seasons equally determinate, as in the days of Hippocrates ; it will not perhaps be hazarding too much to affirm, that they will be found to differ in phænomena, and intensity, from those described by that

¹ An ingenious dissertation on this subject, addressed by Dr. Mitchil, of Washington, in a letter to Dr. Valetine, of Marseilles, will be found in the New York Medical Repository.

eminent physician, in the precise degree, other things being equal, of the difference of cultivation of the soil. Their retrogradation, in this respect, under the malign influence of a barbarous policy, will, in a great measure, account for the increased, and increasing, severity of pestilences in Egypt, Greece, and other countries, which, from having formed a part of a once great, and enlightened nation, are groaning under the devastating yoke of a super-hideous despotism.

It is now, as then, when the Nile, becoming dry, exposes a slimy surface to the action of the sun, and air, by which the properties of the atmosphere are rendered more than usually noxious, that the inhabitants are attacked with pestilential maladies, in Egypt; and, it is now, as then, when the waters of that river begin to re-occupy their bed, that these pernicious exhalations cease, the air resumes its usual purity, and pestilences disappear.

The frequency, with which, in modern, as well as in ancient times, Spain, Poland, the *ci-devant* Venetian States, and the least cultivated parts of the Austrian dominions, as Transylvania and Hungary, have been the theatre of these devastating maladies, is clearly referable to the same cause. In these countries, cultivation has long been either stationary, or retrograde.

If, until within two centuries, the occurrence of yellow fever, the form of epidemic most frequent, and most fatal, in America, and the West Indies, has scarcely, if at all, been recorded, it could only have been, because these countries had not then been long known to, and not much peopled by, Europeans. But, that the original inhabitants of those regions must, before the period to which their historical records extend, have been equally liable to epidemic diseases, and in the very same manner, as they have been, in subsequent times, with the events of which we are better acquainted, the uniformity of the laws of nature forbid us to doubt.

Some new circumstances, in the progress of society, which have occurred within that period, have also contributed to render epidemic diseases, if not more frequent, at least more extensive, and more fatal, than they would otherwise have been, under similar states of cultivation of the soil. One to which I more particularly allude, is the increase in number, and extent, of sea port towns. A great many even in Europe, and almost all the cities of this description, of any consequence, in America, and the West Indies, have sprung up within the last three centuries: and it is at a period still later, that the ravages of yellow fever have become so conspicuous in those parts, as to have attracted the attention of the historian.

At all periods, the population of cities, in low and damp situations, as is generally the case of sea port towns, and crowded together, must have suffered more from epidemic diseases, in proportion to its number, than that of the country parts. These observations will apply especially to Grand Cairo, where, according to Savary, the inhabitants are heaped together by thousands. Two hundred citizens there occupy less space, he says, than thirty at Paris. But thirty citizens of Paris, occupy less space, than ten citizens of London. Consequently *twenty* citizens of Grand Cairo, occupy less space than *one* citizen of London¹. This circumstance, although not alone sufficient to produce, must always greatly aggravate, the calamities of pestilence.

But, what would have been the condition, let us, for a moment, figure to ourselves, of this overcrowded city, if its inhabitants would have been persuaded to believe, that epidemic diseases are capable of being propagated by contagion? Its population, as happened to that of the Greek town of Sycion, now the village of Basilika, on the borders of the Black Sea, would, in the

¹ Dissertation on the Source of Epidemic and Pestilential Diseases. Calcutta edition, 1796, p. 35. It is proper to state, that, although the printer has thought proper to affix the date of 1797 to this dissertation, it was actually published in August, 1796.

course of a single pestilence, have been totally destroyed.

This pernicious belief, however, has existed in all the sea port towns of America, and the West Indies, since their first establishment, as it has done in all the Christian towns of Europe, since 1547; and has occasioned twenty times the destruction, in seasons of pestilence, which would otherwise have happened, from the mere operation of a noxious atmosphere, and of the other proper causes of the disease.

A year does not pass, without the occurrence of an epidemic in some country; but, for the most part, several countries are affected at the same time; and it has frequently happened, that a great portion, and sometimes, that even almost the whole, of the habitable globe has been simultaneously, or in rapid succession, afflicted with this calamity¹.

The liability of nations to epidemic diseases alters. Whilst some that were formerly often visited by them, are now become less frequently the theatre of epidemic diseases, as Great Britain

¹ From 1346 to 1350, the plague is said to have dispeopled the earth of more than one-half of its inhabitants. It continued in England from 1348 to 1357, and, in many places, swept away nine-tenths of all classes of people. The mortality has been no doubt exaggerated. But its universality seems undoubted.

and France ; others, which were formerly less frequently their seat, are now oftener visited by them, as the Turkish dominions. These countries have, in this respect, changed characters : and the obvious explanation of this fact, is, the advancement of the one, and the retrogradation of the other, in respect to cultivation, and the arts of life.

If the frequency of recurrence, and the intensity of the proper causes of pestilences, have, upon an average of nations, diminished, in proportion to the progress of civilization, and the advancement of the arts of life, for the last three centuries, there exists no doubt in my mind, that, from the more than equivalent increase of the moral or adventitious causes, within the same period, their calamities, upon the whole, have been considerably augmented, at least in Christian communities.

BOOK X.

OF THE NATURE AND CURE OF EPIDEMIC DISEASES.

“ In universities and colleges, men’s studies are almost confined to certain authors, from which, if any dissenteth, or propoundeth matter of redargution, it is enough to make him thought a person turbulent.”

FIL. LABYRINTH.

CHAP. XLIII.

“ Morborum omnium unus et idem est modus; locus vero ipse eorum differentiam facit.”

HIP. IN LIB. DE FLAT.

OF THE NATURE OF EPIDEMIC DISEASES.

THE nature of all morbid affections is the same, by whatever remote cause they may be produced¹. Epidemic, like other diseases, consist of diminished excitement of one, or more, or several, organs, of the living body, in all the

¹ “ Elements of Medicine;” where these propositions are elucidated.

combinations, proportions, and degrees, in which they can be affected by a noxious atmosphere, and other concurring causes: differing in no respect from affections of the same organs, of similar extent, situation, and degree, produced by contagion, or others of the exciting powers, whether ordinary or extraordinary; as those arising from the misapplication of remedies.

Although, in epidemic diseases of high degree, almost all the organs of the body are liable to participate in the morbid affection, those, which principally suffer, are the circulating, the muscular and glandular systems, and the brain and nerves. The skin, the membranes, &c. are also frequently affected: and, in fact, it cannot be said, that any part of the living body is exempt from the liability of participating in these maladies. The combinations and degrees, in which these systems and organs may participate, in the disease, are, from the very nature of its cause, various, and almost infinite. But, in this, there is nothing so inexplicable, or surprising, as to justify the wonder, and terror, with which these maladies have been beheld; or the habit of regarding the great diversity of their phænomena as depending upon causes altogether mysterious and incomprehensible.

CHAP. XLIV.

“ Antisthenes, being asked what learning was most necessary, answered: “to unlearn that which is nought.”

LORD BACON'S APOPHTHEGMS.

The cure of epidemic, of greater consequence than, that of other diseases—modes of treatment ranked under three general heads; inefficient, pernicious, and appropriate—the pernicious modes are indirect, direct, and mixed.—The first consists in purging and other evacuations; the second in blood-letting; and the third in a combination of both.—Of indirect evacuations.—Of blood-letting.

WHAT regards the cure, is obviously the first, and most essential part of the study of medicine: and its importance is enhanced, in the precise proportion of the severity and fatality of diseases. In pestilences, therefore, independently of adventitious circumstances, a knowledge of the cure is of far greater consequence, than in other maladies: and this consequence is farther augmented, by the impossibility, without such knowledge, under the influence of the prevailing errors, among Christians, respecting their cause, of ap-

plying the methods of treatment usual in inferior diseases, or of ascertaining, by the result of well concerted experiments, others that are more appropriate, or efficient. Hence, the stagnation, or retrogradation, of medicine, in comparison of other sciences, of which I have endeavoured to prove the existence, and to trace the causes, has been most conspicuous and most to be deplored, in respect to epidemic and pestilential maladies.

All the modes of treatment, which can be employed, may, for convenience, be ranked under three general heads. 1. Such as are simply inefficient, and consequently indifferent. 2. Such as are positively pernicious, and ought, therefore, to be avoided; and 3. Such as have been found, from actual experiment, and logical induction, to be efficient, and appropriate.

The means which are simply inefficient, as they merely leave maladies to their own course, do not appear to merit, excepting incidentally, any notice; and much less particular enumeration. But those, which are positively pernicious, in diseases in general, being still more so, in epidemic diseases, in proportion to their greater intensity, and to the high degree of remedial power, of a diametrically opposite nature, which they require, are deserving of our especial consideration.

In investigating the cure of epidemic diseases, it appears necessary, that I should adopt a similar line of discussion, with that which I pursued, in reasoning concerning their cause. I deem it the most perspicuous mode of proceeding, first, to shew, that every mode of treatment, which has been usually pursued, that is not merely inefficient, is not only improper, but directly the reverse of what is proper, and therefore to be avoided as mischievous ; and afterwards to lay down a plan of treatment, which is derived from experience, and founded upon principle, and consequently to be strictly observed. Thus, I shall shew, not according to the preposterous evidence of tradition and testimony, or the bare allegations of authority, but according to the principles of inductive philosophy, as applied, successfully, I think, in the first volume, to refute the doctrine of contagion, that all the modes of evacuation, or abstraction of the exciting powers, whether ordinary or extraordinary, are, in all affections, and most in those of higher degree, directly and powerfully mischievous, and ought, *upon all occasions*, to be shunned like pestilence itself. The pernicious errors, which have prevailed, respecting the cause of epidemic diseases, have been shewn to be only between two and three hundred years old ; whilst those, which have prevailed, respecting their cure, will be

found to have existed at least between two and three thousand. But, as antiquity cannot render truth more true, so, it cannot render error less erroneous: and, in proportion to the veneration which may have been conferred upon pernicious errors, by the adventitious credit of a high antiquity, the more essential is their refutation.

The pernicious modes of practice, which ought to be avoided, are principally of two kinds. The one consists in the application of the extraordinary exciting powers, as remedies, in such a manner as to produce *indirectly* diminished excitement, or disease; the other, in the subduction of certain portions of the ordinary exciting powers¹, so as to produce *directly* a similar effect. There is also a third mode, which consists of a combination of the other two.

It is by the cessation of the action of the extraordinary exciting powers, previously applied, that they occasion diminished excitement, or disease. This is the invariable effect of the mode, in which they are usually administered as remedies. "Head-ach does not instantaneously follow the application of spirituous liquors; delirium, or fever, the application of opium, or the solar

¹ The distinction between the ordinary and extraordinary exciting powers, is stated in "A View of the Science of Life," p. 5. and will be more fully explained in my "Elements of Medicine."

rays ; vomiting, the application of tartar emetic ; salivation, the use of mercury ; purging, the exhibition of cathartics ; sweat, of sudorifics ; nor vesication, the application of a blister, or of fire, to the skin ¹." That these phænomena indicate a deficient excitement, or disease, is proved by the manner of their removal, which is by the re-application of the exciting powers, by the diminution of whose action they have been occasioned, or of others equivalent in force. All modes of administering remedies, by which the excitement may be diminished, ought to be carefully avoided.

The second mode of practice, which is capable of being shewn to be pernicious, in a still higher degree, is, that which consists of the abstraction of the ordinary exciting powers, as the blood. Others, as air, heat, light, food, may also be occasionally withheld, to the injury of health, or destruction of life ; but, it is so rarely, that this negation, excepting in respect to food, is practised in a remedial view, that it scarcely forms an object worthy of consideration. Blood-letting, however, is practised so frequently, and to so great an extent, with a curative intention, that, from its peculiarly destructive effect, it is deserving of the strictest investigation. The following remarks, upon the subject, although, for reasons

¹ Postscript to "A View of the Science of Life," p. 121, 2.

which it is not necessary to state, they have not hitherto been published as a whole, were written many years ago, with the exception of those parts at the end, which have been suggested by the contradictory practice, forming the third mode of exceptionable treatment, as it has been very generally followed in the recent epidemics of the Spanish peninsula, and other pestilences of a late date. I mean the combination of blood-letting with mercury.

As the invariable effect of diminishing excitation, in the state of health, is to produce disease, and, in the state of disease, to augment the malady ; so, no exciting power can, at any time, be diminished, or withdrawn, without producing either of these consequences.

Either, therefore, the abstraction, or loss of blood, must, in every case whatsoever, be pernicious ; or, the diminution of that stimulus must increase, by a positive increase of action, the excitement ; or the whole of this doctrine must be erroneous.

It has been shewn that the evacuations of purging, vomiting, vesication, and ptyalism, which, as well as hæmorrhages, have been preposterously considered as modes, adopted, by nature, to expel morbid matter ; and, on this ground, were probably first adopted, and afterwards continued for the treatment of diseases, are in reality nothing

more than the signs, or symptoms of diminished excitement, of the organ, or organs, which supply, or contain, the fluids evacuated.

The proper effect, however, of the action, the cessation of which is succeeded by these evacuations, is always to increase the excitement; and this increase does often more than counterbalance the diminution, occasioned by the subsequent cessation of action. Upon this principle, remedies, called cathartics, prove of some utility, in dysentery, even according to the ordinary method of administering them; *i. e.* not in consequence, but in spite of the evacuations, which succeed their application in that manner. The degree of beneficial effect, which is, in this case, produced, results, then, from their properties, not as purgatives, but as appropriate exciting powers of the intestines; and cures would be much more certain, rapid, and decisive, if they were administered, in doses, repeated at such intervals, as to maintain an uniform excitation, so that no cessation of action, and consequently no evacuation, could take place.

With respect to evacuations of blood, their injurious consequences are still more palpable and decided. This fluid, which not only pervades, as the appropriate stimulus of the circulating system, the whole of the animal frame, constituting an integral portion of it, but serves the

double purpose of transmitting, to all parts of the body, the principles imbibed from the air, in respiration, and of conveying to the air some of the animal excretions, is, in its nature, essentially vital. Its abstraction, whether by hæmorrhagy, or surgical art, is a directly and powerfully debilitating operation, accompanied by no circumstance, capable, as in the cases of the other evacuations, of counterbalancing, in any degree, that pernicious effect. It is a stimulus, which cannot, like most others, be replaced at will; but for which we must wait the slow progress of chyli-fication.

If the doctrine be true, then, that disease is only to be removed by an increase, and increased by a diminution of excitation, it will follow, as an inevitable consequence, that the abstraction of blood, which constitutes a direct diminution of excitation, will, in every case of disease, augment the malady.

Although enough, I think, has been said, to convince every reader, whose prejudices are not immoveably rivetted by habit, of the pernicious tendency, in all cases, of blood-letting, of which, indeed, the general integrity of the preceding theories might be accepted as a virtual proof; yet my experience of the obstinacy, with which the bulk of mankind are wont to adhere to the most palpably absurd or erroneous doctrines,

especially such as they have been accustomed, in the exercise of their vocations, daily to apply to practice, tells me, that it is not always enough, to demonstrate, in mere logical terms, the erroneous nature of such doctrines, and the mischiefs which their application to practice is capable of producing. To the force of simple probation, it may frequently be necessary, in such cases, to join the power of ridicule.

To the ordeal of these combined tests, then, I hold it right to submit the merits of the practice of blood-letting; which, in its effects, I consider, with the exception of the doctrine of contagion only, the most important and pernicious error, in the extensive range of medical dogmas.

Of all the modes of debilitating, which have ever been exercised upon the frame of man, that, by evacuation, is the most obvious and direct; and, of all the modes of evacuation, that, by blood-letting, is the most impressive, rapid, and intense. Hence, in every case, in which considerably debilitating means have been considered proper, it has been regularly and largely resorted to. Consequently, if the doctrines be true, from which it is a necessary inference, that, in all cases of disease, blood-letting is injurious, it must, under the ordinary practice, very frequently have proved fatal.

The origin of this practice, if it were a matter

of any consequence, it would probably be now impossible precisely to ascertain¹. By whatever circumstances it may have been, at first, suggested, and at whatever time it may have commenced, there is historical evidence that blood-letting has been in use, from the earliest period, to which medical records extend. The immediate subject of our present enquiry, however, shall be, the various grounds upon which this practice has been continued, in various diseases, in latter times, since investigations into the laws of nature have been less shackled by prejudices; and the consequences which have resulted from its employment.

The readiness, independently of medical hypotheses, with which blood-letting has always been resorted to, both by patients and practitioners, may, I think, be thus, in a great measure, accounted for. There appears to be, in the human mind, a principle, by which the sick require striking operations, to satisfy them that something is doing for their relief; and another, by which the medical practitioner, in order to

¹ In the first volume, an origin is assigned for this practice, from Le Clerc. Whether it be the right one, or not, the reader will probably be little solicitous to ascertain: since, by whatever animal, or person, it may have been invented, or introduced, it will be shewn to be monstrous in conception, and fatal in effect.

retain and to extend his practice, is not unwilling to gratify this desire of his patients, especially while he esteems it not injurious. Phlebotomy, and other chirurgical operations, are attended with a certain degree of vulgar eclat. The perceptible effects of emetic and cathartic medicines augment the faith in their utility. The vesication, discharge, pain, stranguary, &c. which follow the application of blisters, are calculated, without reference to their salutary or pernicious consequences, to increase their importance in the public eye. Even the reputation of clysters is considerably enhanced, by that appearance of mysteriousness, which accompanies their administration; and, if a remedy could be found, capable of producing tetanus, or convulsions, at pleasure, without endangering life, it would doubtless be prized, whatever might be its curative merits, in a much higher degree, than any medicine we yet possess. It is to this disposition of the human mind, by which striking effect, or the awe of mystery, is required to inspire or augment faith, that the dogmatist, as well as the empiric, have, in the absence of better principles, been accustomed to apply their skill; having, doubtless, observed, how much less confidence patients are apt to repose in remedies, even of the most salutary tendency, whose action is unaccompanied by grand perceptible operations;

whilst, on the other hand, it could not have escaped their notice, that, the accredited routine of blood-letting, vomiting, blistering, and purging, having been duly performed, whatever be the event, the reputation of the practitioner does not suffer, and, should the patient die, the relations console themselves that every thing possible has been done, attributing the fatal issue solely to the necessity of his fate. A mode of practice thus generally accredited, however ill-founded, or even obviously contrary to reason, acquires, in time, an authority, which it would be unsafe, in a view of professional interest, to resist. In that view, it is less dangerous to let blood, in the cases, in which it is authorised by custom, even if the patients should die in consequence of it, than to omit the operation, if, by that omission, they should unquestionably recover. In such cases, then, the dread of censure may be presumed, sometimes, to operate so powerfully, upon the mind of the practitioner, as, in conjunction with the other motives mentioned, to induce him to enforce, contrary to what would have been his unbiassed judgment, the customary processes of evacuation, in all their energy. When, to the bias thus imperceptibly formed by interest, are added the prejudices of early education, in a still conjectural art, and the influence of erroneous but established opinions, it ought not to surprise,

however it may afflict us, to find a mode of practice still pursued, consisting of every species of evacuation, and every mode of debilitating, as if it were the constant and obvious tendency of the living body, to produce all its functions in excess¹.

¹ A Treatise on Blood-letting in Fevers, by Professor Van Rotterdam, of Ghent, and translated by Dr. Taylor, just published, contains a copious disquisition of all the indications, and contra-indications, of this practice, according to the notions of the schools. The manuscript of this work was communicated to me, and my advice asked concerning its publication. Although, as I informed Mr. Reed, the publisher, I was preparing for the press doctrines of a diametrically opposite nature, my opinion was, that this work should be allowed to go forth; believing that truth can never be injured by discussion, and that, in a view of trade, it might not be an imprudent speculation.

CHAP. XLV.

Some errors, handed down from age to age,
Plead custom's force, and still possess the stage.
That's vile—should we a parent's faults adore,
And err, because our fathers erred before ?

CHURCHILL.

Of the injurious consequences of Blood-letting—erroneous grounds of its continuance—Sect. I. Blood-letting practised, in imitation of supposed efforts of nature—Sect. II. In consequence of hypothetical doctrines respecting inflammation—Sect. III. On account of supposed plethora, or an excessive quantity of blood—Sect. IV. Under the idea of a vitiated state of the blood—Sect. V. Upon the presumption that it relieves pain.

SUCH may have been, in some measure, the origin and progress of blood-letting. But, in the course of time, as objections were occasionally urged, by individuals, against this practice, it was found necessary, by its advocates, to invent hypotheses, and to assign reasons in its favour. These, as they constitute the grounds of its continuance, shall be examined under the following heads :

1. It has been assumed that the spontaneous evacuations, called critical, which happen in diseases, are efforts of nature to expel morbid matter : and it has, from thence, been inferred, that, in imitation of nature, we should attempt to cure diseases by similar evacuations.

2. From the hypothesis, that inflammations are occasioned by an excessive quantity, and an excessive impetus of the blood, in the vessels of the parts affected, necessarily arose the other hypothesis, that abstraction of blood constitutes the proper cure of these diseases.

3. The doctrine of plethora, or an excessive quantity of blood, indicated, of course, a diminution of the volume of that fluid, for the cure of the diseases, supposed to be occasioned by such a state.

4. A bad quality, or vitiated state of the blood, according to the doctrines of the humoral pathology, has also been supposed to require this evacuation.

5. Blood-letting has been asserted to relieve pain, and consequently recommended in diseases, of which pain is a prominent symptom.

6. It has also been recommended, in cases of a supposed stagnation of the vital fluid, and in what has been called a *determination* of it to particular parts, upon the ground of alledged experience of its utility.

7. Upon such grounds, has blood-letting been practised, in almost all diseases. But it has also been practised (periodically) in health, in order to prevent disease.

8. The application of blood-letting to the treatment of particular diseases, has been invariably marked by uncertainty and indecision, as well as by inconsistency and contradiction, both in respect to doctrines and to practice.

These shall now be severally considered, in their proper order.

SECTION I.

In Imitation of supposed Efforts of Nature.

It is elsewhere shewn¹, that evacuations, as purging, salivation, &c., which succeed the application of remedies, are signs, or symptoms of disease, arising from the cessation of their action. That hæmorrhages, and other spontaneous evacuations, are, in like manner, signs, or symptoms, of disease, arising from a similar cause, is obvious, from their being prevented and cured, by the due and continued action of various exciting

¹ These principles will be found stated, and explained, in my "Elements of Medicine;" and farther illustrated in the Reports of Cases, which I have now decided shall be published in a separate volume.

powers, as well as by their being produced by the cessation of the action of the same remedies. But these evacuations, which are always signs of disease¹, have been regarded, by many, as salutary efforts of nature, to expel morbid matter, and accordingly imitated, in the treatment of diseases. It cannot, I should think, be necessary, after what has been said, to enter into any formal refutation of the noted doctrine of Stahl, that “The human body is much disposed to a plethoric state, and consequently to many disorders, which nature (the *vis medicatrix*,) endeavours to obviate and relieve, by exciting hæmorrhagy; that this, therefore, is often necessary to the balance and health of the system; that it is accordingly to be generally encouraged, sometimes solicited, and is not to be suppressed, unless when it goes to great excess, or happens in parts, in which it may be dangerous².” This doctrine is about as reasonable as the popular one, which inculcates the utility of cherishing gout, as a preventative of other diseases; or, that, which would recommend perpetual intoxication, in order to preserve health. On the contrary, hæmorrhagy, being always unquestionably a symptom of dis-

¹ Diarrhœa differs in no other respect from purging, and hæmorrhagy from blood-letting, than in permanence and intensity.

² Cullen's First Lines, par. 777, Reid's edition, of 1802.

ease, and incapable of preventing or curing any disease, higher in degree, than that, which it represents, ought, upon every occasion, to be suppressed, without hesitation, or delay. The casual occurrence of diarrhœa, or hæmorrhagy, towards the termination of a fever, and the actual cessation of that disease, can never be in the relation of cause and effect; because the latter, as a disease of higher degree than either of the former, cannot be displaced by them; and, as they cannot cure even diseases of a lower degree, but by taking their place, it does not appear that our situation would be any wise bettered, by the interference of these presumed salutary efforts of nature. The fact is, that this succession of phænomena merely indicates the change, in consequence of progressive operations of the living functions themselves, of one degree of excitement into another, less dangerous, either in the same, or in some different organs. But a different succession of phænomena may also take place, and the severer be substituted for the milder disease: fever may be substituted for diarrhœa or hæmorrhagy. Now, it does appear that the *vis medicatrix naturæ*, or archæus, if he knew his business, would not suffer this latter change to take place; and that, if he knew it well, he would take a shorter way of curing fevers, if he could not prevent them, than by

exciting a diarrhœa or a hæmorrhagy. The admission of this non-entity, indeed, is no less unphilosophical in reasoning, than a reliance upon its imaginary operations, is injurious in practice.

SECTION II.

In consequence of Hypothetical Doctrines respecting Inflammation.

It having been shewn¹ that inflammations, in common with other diseases, consist of languid excitement, differing simply in extent, situation, and degree; and that the abstraction, or loss of blood, is, in every case of disease, pernicious¹; it follows that blood-letting is pernicious, in every species of inflammation. This might seem sufficient for the present occasion. But, as, upon the doctrines, which have obtained, respecting this disease, has depended, in no inconsiderable degree, the prevalence of the practice of blood-letting, it may be not irrelevant here, to offer a few farther remarks, upon these doctrines, as connected with this practice.

Inflammation has been generally attributed to an increased quantity, and an increased impetus

¹ In "A View of the Science of Life," and to be farther elucidated in my "Elements of Medicine."

of the blood, in the vessels of the part affected. Dr. Cullen's opinions, on medical subjects, being still, perhaps, for obvious reasons, the most extensively diffused, in this country, I shall, after giving his idea of the nature and cause of inflammation, in his own words, adopt it as the text of my commentary: "Some causes of inequality in the distribution of the blood, may throw an unusual quantity of it upon particular vessels, to which it must necessarily prove a stimulus. But, further, *it is probable*, that, to relieve the congestion, the vis medicatrix naturæ increases still more the action of these vessels; and which, as in all other febrile diseases, it effects by the formation of a spasm on their extremities¹." In the next paragraph he thus proceeds: "A spasm of the extreme arteries, supporting an increased action in the course of them, may, therefore, be considered as the proximate cause of inflammation; at least in all cases not arising from direct stimuli applied; and, even in this case, the stimuli may be *supposed* to produce a spasm of the extreme vessels²." That an unusual quantity of blood, arising, we know not how, should rouse the vis medicatrix naturæ, residing, we know not where; that this imaginary superintendant should immediately direct a

¹ Cullen's First Lines, Reid's edition, of 1802, par. 244.

² Ibid. par. 245.

spasm of the extreme vessels to take place ; that this spasm should induce an increased action of the heart and arteries ; and that this increased action should remove a congestion in the vessels of an inflamed part : these may be all very pleasant, or, if you will, very ingenious fancies ; but they are nothing else.

Adhering to plain matters of fact, we shall find that, the circulation of the blood, instead of being more rapid, is slower than ordinary, in the vessels of inflamed parts ; and that the quantity of that fluid has nothing whatever to do with the disease, which is equally liable to happen to those who have little, as to those who have much blood ; to the fat, as to the lean ; to the weak, as to the strong. Plethoric, and emaciated persons, who have equally a deficient quantity of blood, are as liable to peripneumony and rheumatism, as the firm and vigorous, whose vessels contain that fluid, in greater abundance. Again, if a part be subjected to intense heat, as hot water, or the fire, but withdrawn before disorganization takes place, and, if no other exciting power be immediately applied, inflammation will very soon arise, in consequence of the cessation of the previous high action ; whereas, if an exciting power of about, or nearly, the same degree of intensity with that of the heat withdrawn, be immediately applied, and duly continued, no

inflammation of the part will follow. This is the nature of burns and scalds. In the same manner, does inflammation take place, in consequence of the application of blisters, or other exciting powers of intensity, to the skin; and, on the same principles, may it be prevented. It does not, then, appear that the quantity of the blood has any thing whatever to do with the matter. Should the vessels of the part, thus exposed to the causes of inflammation, have previously contained a deficiency of blood, the disease would equally follow. We can, indeed, conceive that, after inflammation has been established, an increased quantity of blood may be received into the vessels of the part affected, owing to their dilatation, from debility. But, as this additional quantity does not constitute either the disease, or its cause, but is obviously an incidental effect, any attempt to remove it, either by general, or local evacuation, could serve no useful purpose; but would, on the contrary, be pernicious.

SECTION III.

On Account of supposed Plethora, or an excessive Quantity of Blood.

The ground, which we shall next examine, upon which blood-letting has been recommended

and practised, is what is called a plethoric state of the body ; or that state, in which the quantity of blood is supposed to be excessive. The blood being a portion of the living animal body, as well as bone, muscle, and the other constituent parts, and differing from them principally in consistence, it is difficult to conceive, that, the blood, any more than those other parts, can, in a sound state, exceed its relative proportion to the whole. But we never think of taking away a portion of bone, muscle, or nerve, on account of any disproportion of bulk, which they may seem to possess, in a sound state, over the rest of the body. It is only when locally diseased, that it can become necessary, or proper, to remove any part of them. Reasoning, thus from general analogy, as we are unable to perceive how too great a quantity of blood can more readily exist, than an excessive proportion of any other component part of the living body ; and, as, when such disproportion between the other parts have existed, no one has ever thought of restoring the equilibrium, by removing a portion of that which was excessive ; the same rule should, in reason, equally apply to, the blood. If, indeed, custom had not rendered us blind to the absurdity of the measure, it would appear to us equally ridiculous to take away a quantity of blood, on account of any seeming excess, while the mass remained in

a sound state, as to diminish the size of the bones, when disproportionate to the muscles, or of the muscles, when disproportionate to the bones. Could such excess, in reality, take place, the remedy would be easy : diminish the quantity of nutriment. But were it undoubtedly true, that the blood is sometimes generated in excess, this could never occasion the diseases that have been attributed to it, since (if there were no other reason,) the undue increase of that fluid, depending upon the slow progress of nutrition, could not happen, with such rapidity, as health, in such cases, usually changes into disease. It is, on the contrary, an acknowledged fact, that, the diseases, which have been generally supposed to arise from an excessive quantity of blood, are occasioned more frequently by a deficiency of that fluid, than by the diminution of the action of any other exciting power. How often have fever, epilepsy, tetanus, delirium, and insanity, arisen from direct loss of blood !

Upon the various grounds now assigned, as well as those, which shall be hereafter stated, the practice of blood-letting has become so general, in almost every form of disease, that, if the theories delivered in this work be true, it must have constituted one of the most fertile sources of destruction to the human species. It has not, however, been without censurers and opponents.

Among the ancients, we find that *Ætius*¹ and *Oribasius*² are afraid of it, even in the case of plenitude; and *C. Aurelianus*³ lays it down as a general rule, “*phlebotomiam nihil jugulatione differre ratio testatur.*” More recently, *Van Helmont* declares all blood-letting to be butchery. But *Botallus*, on the other hand, enjoins it in all possible cases, not even *dropsy* excepted; and so prevalent, it appears, were the notions of *Botallus*, that *Boerhaave* thinks it necessary to censure the bad custom, encouraged by some physicians, of bleeding “old people, *after the age of seventy*, for the cure of a *PLETHORA*.”

During the plague of London, in 1665, disputes ran very high between the chemical and Galenical doctors: and it appears, that if the latter were, perhaps, less absurd in their reasoning, the former were probably less dangerous in their practice. One of them considers the “bleeding and deletery purgation” of the Galenists “more destructive to poor mortals, than all the pests that ever reigned since the creation⁴.” *Dr. Cullen*, who is in general an advocate for this evacuation, admits that blood-lettings have a tendency to increase plethora, and therefore are improperly employed to prevent it⁵. With respect to

¹ 2. 1. 96.² Synop. 7. 26.³ 2. Acut. 33.⁴ Thomson's *Loimotomia*, London, 1666, p. 68.⁵ First Lines of the Practice of Physic, par. 787.

the term "*plethora*," I must here observe that I have not been able to ascertain, that any precise idea is invariably annexed to it, in medical reasoning. If an excessive quantity of blood in the vessels be meant, it appears necessary to enquire, what constitutes an excessive quantity of blood in the vessels? No quantity, as it appears to me, can be excessive, which the vessels are able to contain and to circulate; and it is beyond my comprehension, that any greater quantity, than this, can be received into them, in the usual course of the circulation. But, if, by the term "*plethora*," as I rather suppose is more frequently the case, be meant obesity, or fatness, it is a fact, well known to butchers and graziers, that blood-letting will increase, instead of diminishing, that state. Accordingly, this is a very common process, in some countries, at particular seasons of the year, in order to put cattle in good condition, for sale, or slaughter. And in order to obtain veal that is fat and white, calves are said to be repeatedly bled, until little more than the crude chyle itself, continues to circulate through their vessels. Fat animals have much less blood than lean, in proportion to their weight. It may be seen, in butcher's shops, how few blood-vessels appear in the fat.

SECTION IV.

Under the Idea of a vitiated State of the Blood.

Let us now consider the propriety of blood-letting, in cases, in which the quality of that fluid is supposed to be bad. If the blood be wholly vitiated, the solids, which it supplies with nourishment, must become proportionally so ; and, in that case, the application of medicines, calculated to produce a general alteration on the body, would seem a more reasonable expedient, than the extraction of any portion of either fluids, or solids, of which the whole are in a diseased state. Again, if it were possible that the general mass of circulating fluids should remain sound, while a certain portion of them becomes diseased, we must find some means of inducing that portion to separate from the rest, before we can hope to derive any benefit from phlebotomy. But, as it does not appear that the blood can ever be partially vitiated, while it remains in the course of the circulation ; or, if it could, that it is in our power to separate the bad from the good, I am utterly at a loss to conceive, how blood-letting, in any of these supposed cases, can be deemed useful, or even thought consistent with common sense.

SECTION V.

Upon the Presumption that it relieves Pain.

It has been alledged that, in fevers, peripneumony, coup-de-soleil, inflammations, and, in general, in those diseases, in which blood-letting has been most frequently employed ; immediate relief from pain has followed that operation. As far as my own observation extends, I have never known this circumstance to occur, but when it might be honestly attributed to the imagination of the patient, according to the principle already explained, of the effect of blood-letting, and other operations, upon the mind. There is also another principle upon which this alledged fact may be admitted, and explained, consistently with the general conclusion, that blood-letting cannot, in such cases, be either useful or innoxious. The abstraction of blood, by diminishing sensation, will undoubtedly lessen pain. But the pain thus transiently diminished, or suspended, returns with returning sensation. Neither the disease, or the pain, for which blood-letting may be employed, are ever removed, either at once, or in consequence of a repetition of that operation. But, on the contrary, when the process has been carried to a considerable extent, the patient, if he has the

good fortune to escape with life, falls into some other disease, or experiences a painful and lingering recovery. That a diminution, or cessation of pain, arises from a diminution, or cessation of sensation, is well illustrated by a phænomenon, which sometimes occurs immediately before death. According to the progress, towards mortification of organs, in which pain had previously been very acute, it gradually abates, and, many hours before death, entirely ceases. The phænomena attending syncope, from loss of blood, are equally conclusive. Any pain, under which the patient may have previously laboured, will, for the time, be suspended ; but only until the return of sensation.

CHAP. XLVI.

Hypotheses upon which the practice of blood-letting has been continued.—Sect. VI. On account of a supposed stagnation, or determination of blood to particular parts.—Sect. VII. Practised in health, in order to prevent disease—class of bleeders.—Sect. VIII. Marked by uncertainty, and indecision, inconsistency, and contradiction.

SECTION VI.

On Account of a supposed Stagnation, or Determination of the Blood, to particular Parts:

IT has been usual to connect some local, as well as general affections, not only with a supposed excess, but also with a supposed stagnation, or determination of blood, to particular parts: and hence the continuance, at least, of the practice of blood-letting, upon the principles of the noted doctrines of derivation and revulsion. It has already been shewn, that there exists no ground whatever for attributing the redness, heat, swelling, &c., which constitute the symptoms of inflammation, either to an increased quantity, or an increased impetus, of the blood; that they

arise in consequence of the cessation of the action of certain exciting powers, as caloric, blisters, &c.; and that, if more blood be received into the vessels of the affected parts, than was contained in them before their inflamed state, this is an effect, not a cause, of the disease. In the same manner do redness, and pain, and even a certain degree of swelling, take place, in a very cold day, in those parts, as the hands and face, most exposed to the influence of the atmosphere. But these phænomena, in the latter case, will hardly be attributed to any increase in the quantity, or the impetus of the blood, or to any stagnation, or determination of that fluid to the parts affected; seeing that they are, so directly, and unequivocally, the result of the diminution of the action of heat, on the surface.

There is fully as little reason to attribute any disease, local, or general, as headach, the pain in pleurisy, apoplexy, &c., either to a stagnation of the blood, or to a determination of it to particular parts. With respect to the latter, it has been already shewn, that, when an unusual quantity of blood has been distributed to any particular parts, as is supposed to take place in local inflammation, this is the effect of a dilatation of the vessels, from diminished excitement, admitting then more than their ordinary capacity is adequate to receive, of the vital fluid; not the

cause of that diminished excitement. The same reasoning equally applies to a stagnation of the blood. This phænomenon must be owing, either to a diminished excitement of the blood-vessels, whereby they are unable, from the ordinary excitation of the blood, to propel it with the usual vigour, or to a diminished exciting power of the blood itself, whereby it is incapable of acting, with the usual intensity upon the vessels; or to a combination of both. And any diminution of the quantity of that fluid, instead of increasing the exciting power of the remainder, or the excitement of the blood-vessels, would, it is obvious, diminish both; and consequently augment the disease.

Such are the notions, upon which appears to have been founded the practice of blood-letting, as connected with the celebrated doctrines of derivation and revulsion, which, in the beginning of the fifteenth century, gave rise to a controversy, which, says Freind, “distracted the judgments of all the physicians in Europe, about bleeding in the *direct* or the *opposite* side, in a pleurisy. They (the Arabian physicians) followed, it seems, the opinion of Archigenes and Aretæus, and inclined to the latter practice; and, for that reason, were railed at as revolvers from the doctrine of Hippocrates and Galen, though neither of these lays down any constant unvariable rule in

this point. The university of Salamanca, indeed, took part with the Arabians, and made a decree, that no one, in this case, should dare to let blood but in the *contrary* arm; and to add authority to their decree, they endeavoured to procure an edict from Charles the Fifth to second it, alledging the other method to be of no less ill consequence, than that of Luther's heresy '."

Upon the merits of this extraordinary controversy, I hold it quite unnecessary, at this day, to offer a single comment. But, before dismissing the present division of my subject, it may be proper farther to notice, in a succinct way, a few erroneous grounds, connected with it, upon which the practice of blood-letting has been cultivated, and extended, in various diseases. If a person be exposed to the excessive heat of the sun, so as to experience a coup-de-soleil, a turgid countenance, laborious respiration, and other symptoms, will take place, which it is customary to connect with an excessive quantity of blood, or a determination of it to, or stagnation in, particular parts, as their cause. The symptoms, which succeed a series of excessive drinking, are similar, but milder in degree; as are those, which succeed a violent fit of passion, indicating apoplexy, but severer in degree. Now, it is quite obvious,

† Freind's History of Physic, vol. ii. p. 214.

that the persons so situated, instead of having been labouring under what is usually called a plethora, may have all previously possessed a deficient quantity of blood. We must, then, suppose, in order to justify blood-letting on the plea of excessive quantity, that, in the first case, it has been suddenly increased by exposure to the sun ; in the second, by the liquors swallowed ; and, in the third, by anger. But, the influence of these powers is notoriously of an opposite tendency. Much exposure to heat emaciates the frame : frequent ebriation occasions a deficiency of the vital fluid : and the angry passions are unfriendly to a plethora.

As little can blood-letting be justified, in any disease, upon the supposition that it is occasioned by a stagnation, or determination of the fluid to some particular part. For, these phænomena, as has been shewn, are but effects, not causes, of disease ; and, by the abstraction of any portion of the blood, could only be still farther augmented.

In the diseases last mentioned, as well as in the case of persons apparently drowned, and some others, the practice of blood-letting appears to have been greatly encouraged, by an erroneous inference, deduced from a very simple fact. It has been observed, that, when, upon opening a vein, during a paroxysm, or the apparent sus-

pension of animation, the blood flows freely, the patient generally recovers, at least from the paroxysm ; but that, when it does not flow at all, he dies. Whence, it has been inferred, on the one hand, that recovery takes place, in consequence of the abstraction of the blood ; and, on the other hand, that death happens, in consequence of its non-abstraction. But this inference is palpably most erroneous : for, the flowing of the blood is a phænomenon, which merely indicates that the circulation is still performed, and that a sufficient degree of general excitement probably remains, to produce a solution of the paroxysm ; whilst the non-flowing of the blood indicates that the circulation has ceased, and that there is not probably a sufficient degree of general excitement remaining, without external aid, to produce a solution of the paroxysm.

SECTION VII.

Has been practised in Health, in order to prevent Disease. Class of Bleeders.

But it is not in disease only that blood-letting has been practiced. Amongst the least informed part of the community, in many countries, it is periodically performed, in health, in order to prevent disease. It is somewhat remarkable,

however, that in these very same countries, this operation is seldom, if ever, had recourse to, among people of superior rank and education, who must be presumed, from their mode of living, to possess the greatest quantity of blood. The first Portuguese settlers, on the coast of Africa, refined wonderfully upon this custom. Finding the country, as all uncultivated countries necessarily must be, exceedingly unhealthy, they imagined that their diseases arose from the foreign nature of their European blood; and concluded, that, by extracting certain portions of it, from time to time, it would be replaced by blood more congenial to the climate; and that, thus, the whole mass would, in time, be naturalized. The application, to practice, of this strange conceit, as may reasonably be supposed, added not a little to the otherwise destructive effects of an unwholesome atmosphere. But ridiculous as the deductions of the Portuguese settlers may seem to us, the grounds upon which we have been accustomed to bleed, although, perhaps, more solemn in appearance, are only less absurd in reality, because they are more familiar to our contemplation.

In the city of Philadelphia, as we learn, and it is to be presumed in the other cities of America, there is a class of men, called *bleeders*, distinct from the members of the profession. And it has

been stated, upon what appears good authority, that during the prevalence of the yellow fever, of 1793, the number of these bleeders, who escaped the disease, (happily for the rest of the community,) were found insufficient to answer the demand for them.

Were it not superfluous, many more facts might be adduced, to shew the great extent to which the practice of blood-letting has attained, among mankind, without, however, being accompanied by a single instance, in which it can be proved to have been founded upon an unequivocally defensible ground, or to have been succeeded by an undoubted beneficial result. The mere existence of a practice, if it were even universally to obtain, would form no argument in favour of its utility.

SECTION VIII.

The Practice of Blood-letting, marked by Uncertainty and Indecision, Inconsistency, and Contradiction.

It now only remains to be shewn, that the application of blood-letting, to the treatment of particular diseases, has been invariably marked by uncertainty and indecision, as well as by inconsistency and contradiction, both in respect to doctrines and to practice.

It may be remarked, that, frequently, when celebrated practitioners have thought it necessary to have recourse to bleeding, and other evacuations, solely in deference to custom, it has plainly enough appeared, from their observations, that they entertain no very favourable opinion of their utility. Thus, Sydenham, after having duly premised bleeding and purging, used to cure dysenteries with laudanum. “After the third purge,” says he, “the cure depends *entirely* on giving an opiate twice, or thrice, a day ; but in those constitutions of the air, which have a less tendency to promote this disease, *omitting the evacuations above specified*, it may be cured with *laudanum only*’.” It is evident, that this sagacious physician, whilst he speaks decidedly of the use of laudanum, in dysentery, was influenced by custom, or authority only, in premising bleeding and purging. The disease, in its progress, could not have altered in its nature ; and the mild could only differ from the severe dysentery, in degree. If, therefore, Sydenham had not entertained a distrust of evacuations, he would, we may presume, have persevered in them, in the severer cases, throughout ; and, if he had not entertained a confidence in laudanum, he would not have trusted to it exclusively, in all the milder cases,

¹ Swan’s Sydenham, p. 662.

as he appears to have done. It may be farther observed, that, if, in any lower degree of dysentery, a certain quantity of opium be, of itself, sufficient to effect a cure, in every higher degree of the same disease (that is not incurable,) a quantity of opium, proportionally greater, will, of itself, be also sufficient to effect a cure. From this I would not be understood to give it, as my opinion, that opium is the most appropriate remedy, in dysentery, although I believe it to be a good one. Before we come to decided conclusions, this subject stands in need of farther research. Of this, however, I am sure, that blood-letting is, in a higher, and other debilitating means, in a lower degree, pernicious, in that disease.

That judicious and candid surgeon, Mr. Bromfield, in treating of concussions of the brain, in which the practice of blood-letting was, in his time, carried to an unheard of extent, expresses his disapprobation of it, in reserved, yet sufficiently intelligible language. "Let truth prevail," says he, "though the present doctrine, enjoining great and repeated evacuations, should suffer a little¹." In those cases, in which he lets blood, he freely confesses the inutility of the practice.

¹ Bromfield's Surgery, p. 24.

In the treatment of diseases, bleeding seems always to have been employed, rather as a desultory measure, from which no precise end was proposed, than as part of any methodical plan of treatment. But, had that evacuation, conducted in this loose and desultory way, been found unequivocally beneficial, would it not, upon the first bleeding proving inadequate, have been thought necessary to repeat it, a second, third, or fourth time, or more, until the disease was cured? Would it not have been deemed proper, in such case, to repeat the operation, until the desired effect was produced, in the same manner as the doses of mercury used to be repeated, in lues venerea, or of Peruvian bark, in fevers. I am not, however, aware, that in any disease, either the quantity to be taken away at once, or the manner in which the operation ought to be repeated, has ever been attempted to be regulated by distinct precepts. The praises of blood-letting have, at all times, consisted of mere general assertions: and, whether the practice has been conducted in a desultory and capricious manner, or according to some arbitrary rules, the annals of medicine do not furnish any satisfactory series of facts, evincing that this evacuation has, in any single case of disease, proved an unequivocal cure. Had it, in any one case of disease, demonstratively effected a

a cure, it would, of course, have been immediately established, as the regular means of treating that disease generally. For, if there be any one case of disease, in which any remedy, employed to a certain extent, does prove an undoubted cure, the same means, used in a proportional degree, must prove equally beneficial, in every other (curable) case of the same disease.

But what, let me ask, are the diseases, in which bleeding can be alledged to have been found uniformly advantageous? Is it in peripneumony, which, from a malady of moderate degree, has been, by that practice, so frequently converted into hydrothorax? Is it in epidemic or pestilential diseases, which require, for their cure, the highest application of the exciting powers? Is it in puerperal fever, with inflammation of the peritonæum, in which so many females annually expire, under the operation of the lancet, that we are to look for the benefits of this mode of evacuation? Let the recorded mortality, in the lying-in-hospitals, in London, and other great cities, farther declare.

It is well worthy of remark, that, whilst there hardly exists a disease, in which, in some of its stages, or under some of its circumstances, certain physicians have not recommended blood-letting, in other stages, and under other circumstances, of the

same maladies, it has been deemed necessary, by the very same physicians, to set forth so many cautions, and prohibitions, respecting the use of this evacuation, that it becomes quite impossible, from a perusal of their precepts, to decide whether they are more in favour of, or against the remedy¹. Is it not altogether irrational, that a remedy, which has not been ascertained to be useful, in any disease generally, should be supposed capable of effecting cures, by its desultory application, in single cases of disease?

The partizans of blood-letting, in almost every case of disease, in which it has been customary with them to employ it, have also, at the same time used blisters, opium, and other stimulant powers, either in concourse or succession; and sometimes, as when the intensity of these powers has been equivalent to the force of the disease, and the debilitating effects of the loss of blood, conjointly, with seeming advantage. But how inconsistent and contradictory, to use, at once, and in the same disease, means of the highest debilitating, and others of the most powerfully

¹ For instances of the uncertainty, and indecision, manifested by Dr. Cullen, respecting bleeding and purging, in fevers, and other diseases, See articles 140, 141, 145, 148, 670, 687, of his "First Lines of the Practice of Physic," edition of 1802.

invigorating, operation ! Even the co-administration, with blood-letting, of the remedies, usually called purgatives, whose proper action has been shewn to be stimulant, is, in reality, a contradiction.

CHAP. XLVII.

The practice of Blood-letting made a theme of factious commendation—supported by inappropriate evidence—inconsistency of its advocates.—Of late brought out anew, with the additional inconsistency of its combination with mercury—misrepresentations, concerning the effects of this contradictory treatment.—Conclusion that blood-letting ought never to be performed.

TO the foregoing observations, on blood-letting, which, as I have said, were written a great many years ago, although never published as a whole, I think it necessary to add a few remarks, in consequence of the very extraordinary mode, which has recently been resorted to, with a view to defend, to justify, and to recommend the continuance of that most deleterious and irrational practice. During the epidemics, which have, of late years, occurred in America, the West Indies, and the Spanish Peninsula, in which diseases, as has been shewn, by fact and argument, this practice must necessarily be destructive in the highest degree, it seems, with a total disregard of all evidence befitting science, to have been made a theme of factious commendation. Nume-

rous, almost innumerable cases, i. e. *assertions*, are published, *in proof* of the incomparable efficiency of phlebotomy, precisely in those very cases of disease, in which reason would teach us that it must be the most fatal. If we are to believe these accounts, there is not a patient, who has been so fortunate as to fall into the hands of a thorough-paced phlebotomist, who has not, almost before the lancet has been withdrawn from the vein, felt all his pains instantly vanish. “Pain of side considerably alleviated;”—“head-ache sensibly diminished;”—“spasms greatly mitigated;”—“gripes wonderfully relieved;” are the recorded effects of blood-letting, in each reported case. This relief, indeed, so invariably, and closely follows this operation, according to the testimony of the operators, that, what must strike those persons who are a little prone to enquiry, as the most surprising part of the whole, is, that under the influence of so never-failing a treatment, any patient should ever die; or, that the advocates of the practice, who, as they say, have had such ample experience of its virtues, should not be prepared to follow it up in all cases, to the instantaneous destruction of every malady, that is not from the commencement decidedly incurable; since it is a mode of treatment, over which there is an unlimited command. For, if the abstraction of ten, twenty, thirty, or forty

ounces, be not found sufficient to effect a cure, the means being considered appropriate, fifty, sixty, seventy, eighty, a hundred, or up to two hundred ounces, ought to be extracted ; and so on, until the cure be completed, or no more blood remains. This would certainly be the consistent mode of proceeding, according to the notions of the phlebotomists themselves. And, upon coming to the point, at which no more of the vital fluid was to be found, (life being supposed to exist,) the disease being still unsubdued, their views ought next to be directed to the subduction of air, heat, light, food, and every other exciting power, until the enemy was totally vanquished. This, I say, would be their only consistent course ; since, as blood-letting, abstracted from those hypotheses, by which its real nature has been concealed, is nothing else than the direct and positive diminution of an exciting power, of the highest degree, its operation could be aided in no other manner, than by the exclusion of one, or some, or all of the other exciting powers, to the degree, which is sufficient to produce the inanition required, in order to remove the malady.

So far, however, have the advocates of blood-letting been from choosing to proceed upon their own principles, that they neither follow up phlebotomy by the subduction of other exciting

powers, nor ever trust to the efficacy of their panacea alone ; but, with a still more egregious inconsistency, combine the practice of blood-letting, with the administration of mercury ; i. e. they have recourse to the subduction of one exciting power, and the application of another, thus employing means of a diametrically opposite nature, at one and the same time ! I know it has been alledged, as a ground for this extraordinary practice, that, by premising blood-letting, more opium or mercury may be *safely* thrown into the system. But it seems rather a strange fancy to extract blood, in order to have the pleasure of throwing more opium or mercury into the system, than would otherwise be necessary ; for no one would surely think of administering more than was necessary, merely because it might be safe. This would be to debilitate, in order to strengthen ; to accommodate the state of the patient to the sum of medicine, that is determined to be given, rather than proportion the sum of medicine to the state of the patient ! On the absurdity of this practice, I had, at a former period, occasion to comment, in the following terms : “ If it were permitted, upon such subjects, to reason in a plain way, I would ask, if a patient’s excitement be five degrees below the healthy standard, how can any rational being think of lowering it five degrees more, that he may afterwards raise

it, with the greater safety? He will then require to apply double the force that would at first have been sufficient ¹.”

It is curious to observe, with what eagerness the persons, who resort to this most extraordinary admixture of practice, are ready, whenever a patient happens to survive the ordeal, to give the credit of what they call the recovery, to the blood-letting, to the entire exclusion of the operation of the mercury. Here again, they fall into a new course of inconsistencies. For, as blood-letting is, according to their own notions, as well as in truth, a debilitating operation; and, as mercury, which, I believe, they will not deny, is an exciting power of high degree; to administer the latter is to diminish the effect of the former, and a practice, which cannot be justified, according to their own, or to any other hypothesis. But, if they believe their own assertions, that, the recoveries, which are alledged to be cures, in such cases, are exclusively the effects of the blood-letting, it follows that they consider the mercury as either useless, or injurious; and, in either case, there can be no propriety in its administration. It is as contradictory a practice, as it would be, in a person administering mercury, or any other *exciting* power, to counteract their

¹ Treatise on the Action of Mercury, 1796, p. 18, 19.

effects, by the *debilitating* operation of blood-letting.

Although blood-letting has been in use between two and three thousand years, or longer, for any thing we know, it does not appear to have been carried to such extreme lengths, in the days of mere ignorance, as it has in those of false knowledge. It will now appear that the tales, which have been recorded, concerning its efficacy, are entitled to no more credit than what I have shewn to be due to the traditions, which have prevailed, for the last 270 years, respecting the propagation of epidemic diseases, by means of a specific contagion. Amongst other stories, of a similar kind, may be cited the following, from Turner against Dover:

“An ancient physician had about two hundred and seventy sailors under his care, sick with the plague; they had all of them the spots (called tokens,) and they were seized with langour, insomuch that they were not able to move; he ordered his surgeon to bleed them in both arms, and to go round to them all, with command to leave them bleeding till all were blooded, and then come and tie them up in their turns. Thus they lay bleeding and fainting so long, that the physician could not conceive they could lose less than a hundred ounces a man.

There were but seven or eight lost (not by the bleeding, but) by their messmates procuring for them strong liquors."

Another tale, similar to this, or perhaps another edition of the same, is recorded by Sydenham, as stated in my preliminary discourse. And many might be cited, of equal extravagance, in our own times¹. But any farther exposition of such absurdities, I hold to be superfluous. It may merely be remarked, of the story of the two hundred and seventy sailors (if they ever existed,) that credit was given to the blood-letting for the recoveries, and blame to strong liquors, presumed to have been administered by the messmates of the sick, for the deaths, of which real experience will pronounce them to be equally guiltless. Blood-letting cannot, in any case of pestilence, have any other than a pernicious, nor spirits any other than a beneficial operation: and, if the latter ought never to be trusted to, and very seldom to be employed, it is only because, as being of inadequate intensity, they would not be efficient as means of cure.

All the romances which exist, respecting the efficiency of blood-letting, seeing that we cannot, in modern times, by a repetition of similar processes, produce similar effects, would not, if they

¹ As the practice in the Isle of Wight, stated in the London Medical Observer.

had been sworn to, by every physician, who ever existed, be entitled to any more credit, than the wonderful cures which, we are told, are daily performed, as *per affidavit*, by the numerous quacks of the British Metropolis.

Upon the credit given in medicine to the delusive evidence of tradition, and testimony, and the consequent retardation of improvement in the art, is principally founded the calamitous progress of empiricism in the present times ; which is so pre-eminently destructive and disgraceful to the inhabitants of the metropolis of the British empire. Nothing is more common there than to read assertions upon oath, that persons have been cured of diseases, or that they have cured others, by processes, which the slightest knowledge of the laws of nature determine to be altogether incapable of producing such effects ; and to see thousands, in consequence, hastening to undergo similar treatment, and to be robbed of their money, and their constitutions, and very frequently of their lives. This most destructive evil continues daily to cry louder and louder for the interposition of the legislature ; but by a mode very different from those, which have hitherto been resorted to, for the suppression of quackery.

Of late years, the practice of blood-letting, the most common place, as well as the most deleterious, which has ever disgraced the annals of

medicine, and prevailed for the longest period, without examination, has been brought out, as it were, and with additional eclat, in epidemic diseases, as if it had been a treatment just discovered, or at least as if some novel and ingenious method of employing it had been just ascertained, by which it had been rendered of undoubted and uniform efficacy in those diseases. And, what are the proofs of its efficacy, which have been offered? Facts, and reasoning? Induction of experiment? No such thing: nothing but tradition, testimony, assertion, and authority! After having been in use, for several thousand years, so little is its nature understood, that, those, who contend for its efficacy, are unable to condescend upon the cases, in which it ought to be employed; and cannot fix upon any disease, in which, by any known processes of phlebotomy, we may be assured of producing certain and determinate effects.

It is not my intention, indeed it would be very inconsistent in me, to attempt to give any importance to the testimonies of those authors, who might be cited *against* the good effects of the practice of blood-letting; since their assertions, equally with those of its advocates, have been founded upon the fallacious results of their own presumed experience. Instead of fact and argument, or experiment and induction, which alone

can supply correct medical conclusions, nothing, as I have said, has been offered, in proof, on either side of the question, but the evidence of tradition, testimony, assertion, and authority, all of them alike foreign to the acknowledged principles of philosophical research.

If it could be necessary to use farther arguments to shew, that, neither blood-letting, nor any of the other modes of practice, recommended in ancient, or in modern times, for the cure of epidemic diseases, although some of them might be less positively mischievous than others, could have been productive of any uniform or decided benefits ; this conclusion would necessarily result from a knowledge of the means, which are now ascertained to be really efficient, and of the principles of cure, which are here established. It may, however, serve to put this matter in a more perspicuous point of view, if I quote the acknowledgments (on the unfavourable side such evidence is the less questionable,) of medical writers, who have had opportunities of collecting the alledged results of the medical treatment, employed in the recent pestilences of Gibraltar, and the Spanish Peninsula ; observing that their remarks will equally apply to every history, which has been published, respecting other epidemic and pestilential diseases.

“ From the surviving medical men,” (in the

fever of Gibraltar, in 1804,) says Sir James Fellowes, “ *I could collect nothing consolatory, nor could any of them hold out a prospect of success, from the use of any particular medicine, as efficacious in this disease* ¹. ”

Mr. Griffiths, surgeon of the Leviathan, states, that, in this fever, “ *blood-letting was used upon the hypothesis of inflammation ; but that it had been left off, and he cannot take upon him to state what had been substituted. I believe, however,* ” says he, “ *that nothing availed ; all seemed horror and confusion, towards the end of October, when the efforts of the practitioners were principally directed to prevent the disease extending* ². ”

At page 379, it is stated, from a document of Fra. Lougi di Pavia, of Smyrna, who had presided over an hospital in Turkey, for thirty years, that he had never, during the whole of that period, known *a single case of plague to be cured by the rules of art*. In fact, there has hitherto been no cure for epidemic diseases, whenever they have amounted to the lowest degree of pestilence. But, in their milder degrees, when almost every patient would recover, if left to the operation of the surrounding elements, or

¹ Reports, p. 154.

² Burnet, p. 213. The value of the doctrine of inflammation has been already estimated.

even in defiance of a mode of treatment calculated to augment disease, *recoveries* are represented as *cures*; and the cures attributed to measures not simply inefficient, but positively mischievous. Whilst, in severe pestilences, like that of Gibraltar, in 1804, neither the inefficacy nor the perniciousness of blood-letting are to be concealed, or denied; epidemics of inferior degree, as those of the same place, in 1813 and 1814, have been laid hold of, as fit occasions for giving to that practice the credit of cures, which are only recoveries that could not but have happened, because the disease was not originally of a violence to prove fatal; recoveries that have taken place, in spite of the effects of blood-letting; and which would have proceeded much quicker, if such evacuations had not been performed. If I should be required to enter into a regular refutation of the assertions which have been advanced by writers upon this subject, I would reply, that I am not bound to prove a negative. But I am certainly bound to reject the mere *ipse dixit* of narrators, (and it is not of the smallest consequence to the question whether their testimony be *bona fide*, or otherwise,) when the results they alledge militate directly against nature and reason, and the measures, which they recommend, have nothing but practice and prescription for their support. In accounting for

absurd practices of any kind, I hold the native of Hindostan to be the most genuine philosopher. He refers you, at once, to a ground, which does not admit of dispute. If you were to ask a Hindoo, for instance, why blood is extracted in disease, or any other foolish thing done, instead of bewildering you and himself in mazes of sophistry, to shew that inflammation is a state of excessive vigour, and therefore requires depletion, he would honestly assign, in one word, the true ground of the proceeding, "*custom.*"

In respect to the mild, or what, in medical language, may be called the benign fevers of Gibraltar, in 1813 and 1814, the most extraordinary efforts appear to have been made, to produce a belief, that, the *recoveries*, which took place, were *cures* performed, by blood-letting. A hundred soldiers of a regiment, for instance, are attacked with the incipient symptoms of an epidemic malady, of scarcely more intensity than that which has been known by the name of "*influenza.*" They are, of course, sent immediately to the hospital. If the surgeon be a thorough-paced phlebotomist, they are all bled, perhaps in files. They all, of course, recover; because, as the disease was originally mild, it would, in no case, prove fatal, unless it had been rendered so by the blood-letting: and, as, in mild diseases, there can be no motive, or pre-

text, even according to the ideas of its advocates, for pushing this practice to extremity, it will be very difficult, even under the worst possible treatment, (although instances of it have been known) to render such a malady destructive, in any considerable degree. If the same patients had had each a finger amputated, they would all, no doubt, have recovered: but would they have recovered in consequence of the amputation?

In the fevers of Gibraltar, in the years already mentioned, which I conclude to be of this description, because, if they had been intrinsically severe, it is obvious, from the mode of practice, which has been described, as having been generally followed, that the mortality must have been much greater, the treatment, for the most part, appears to have consisted, either of blood-letting alone, or of a combination of it with calomel, or with some of the other exciting powers usually denominated cathartics: and, as uncommon pains have been taken to mislead the public into a persuasion of the utility of this practice, especially the blood-letting, I hold it necessary, here, to make some remarks upon the subject.

In his "Practical Account of the Mediterranean Fever," Dr. Burnet, (Pref. p. ix.) after pronouncing a due eulogium on the virtues of blood-letting, resorts, for confirmation, to the

usual species of evidence. “ If the *testimony* of the *respectable* gentlemen, whose names appear in this volume, be not sufficient, he can appeal to the admirals of the fleet, under whom he has had the honour of serving, and to the captains of the ships, in which this fever has appeared, for the truth of his assertions.” Now, if it were a question of fact, such as the number of killed, wounded, and missing, or died of disease, that was to be decided, certainly there are few, if any, bodies of men, to whose integrity an appeal might be made, with greater safety, than the admirals and captains of the British navy. But, when it relates to assertions, which involve the most important conclusions, respecting the truth, or fallacy, of fundamental principles in medicine, the testimony of all the individuals in the universe would not constitute an iota of proof. If all the evidence of this kind, of all the persons, who have ever existed, could be brought to bear upon a single point of science, it would be utterly worthless ; since it might be invalidated, not only in the course of a single day, but by a single experiment. This is a proposition, which I have taken the liberty, as deeming it of extraordinary importance, of repeating, in a variety of shapes, in the course of this work, as well as of insisting upon the very pernicious consequences, both to

science and to humanity, which have resulted from the almost universal employment of this inappropriate species of evidence, in medical disquisitions.

At page 402, this author, with an implicit reliance upon the same kind of evidence, observes : “ On examining the *official* replies of the medical officers, who were in the garrison, (Gibraltar) during the epidemic fever of 1814, (for access to which I am indebted to the politeness of Sir James Mac Grigor, and the other members of the Army Medical Board,) I find, *wherever blood-letting is adopted*, that it is *spoken of* in the *most favourable manner*.” And he thus winds up the climax (p. 414): “ It is *consolatory* to know that this practice (blood-letting) is, every where, within the tropics, rapidly gaining ground, and I trust the time is not far distant, when it will be universally adopted.” If the assertion, that this practice is gaining ground between the tropics, were even correct, it would be nothing to the purpose. But I must presume to question the accuracy of Dr. Burnet’s information on this point ; being well assured that, if it were practicable to poll all the practitioners of medicine within the tropics, *who are not under any species of official restraint*, an immense majority of them would be found to have wholly abjured the practice of blood-letting, as highly deleterious, at least in epidemic diseases. I am even persuaded

that, if it were equally practicable, transfusion of blood would be reckoned, in such cases, by a great majority of the most experienced intertropical practitioners, a practice of extreme rationality, compared to that of abstraction. But these are mere matters of fact, which, in respect to private practitioners, it would be difficult, or impracticable, to ascertain; and when ascertained, would not be worth the search, as conveying only a knowledge of prevailing opinions.

Again:—"It is much to be lamented, that blood-letting was so seldom used; as, *from the symptoms, as well as the morbid appearances*, found on dissection, it is *unquestionably* a remedy of the highest value in this devastating disease." P. 309. It is not, either by the symptoms, or the morbid appearances upon dissection, but by the results of their operation, in removing disease, that a just decision can be formed, respecting the value of remedies.

At page 353, of the same work, Mr. Humphreys has this sweeping statement:—"Every patient, but one, labouring under this fever in 1814, who was bled *by me, and under my own immediate charge*, recovered." Besides the ambiguity of this language, which might lead us to infer, that some patients who were bled by him, but not under his own immediate charge, or some, who were under his own immediate charge, but bled

by others, had died, or both ; he omits to state the number of the patients, and what were the degrees of intensity of the diseases, from which recovery ensued, under such treatment, or whether any other treatment was, at the same time, employed !

The same gentleman states, (Burnet, p. 357,) that he has “endeavoured to inculcate the necessity of evacuating blood in the *early* stage of this disease, and of the *depletory*, instead of the *stimulating* system ; and then proceeds to describe the appearances upon dissection, in justification of the practice. It has been fully shewn, in another place, that the phænomena, usually called inflammation, are decided and unequivocal symptoms of debility, or deficient excitement, indicating a mode of treatment, in every respect, directly the reverse of blood-letting ; as well as that diseases cannot, in the course of their progress, alter in their nature ; and consequently that what is *noxious* in the *later* stages, cannot be *beneficial*, in the *earlier* periods, of the *same* disorder.

I have already remarked on the disingenuousness of employing blood-letting and calomel, and of endeavouring to give the credit of recoveries to the former, and the blame of failures, either expressly, or by implication, to the latter. At page 401, of Dr. Burnet's book, Mr. Lea, of the 26th regiment, states the case of Colonel L. to

whom the first thing administered was the cold affusion, afterwards *twenty grains of calomel*, and subsequently blood-letting, to the latter of which the recovery of the patient is attributed! I have heard, without surprise, nurses, and uninformed persons, indiscriminately imputing both illnesses, and recoveries, to the last thing, which patients have taken; but in medical men, this mode of reasoning, although not new, is extremely reprehensible.

At page 359 of the same work, reports of cases are communicated by the surgeon of the Barrosa, in which yellow fever, in May, 1814, was treated by calomel and blood-letting; the *recoveries* stated as cures; and the "cures" attributed to the latter!

From page 380 to 383, we find the surgeon of the Venerable, by way of "a convincing commentary," as he says, "on the practice and principles laid down and inculcated in Dr. Burnet's book," relating his own recovery from an illness, after taking *forty-five grains of calomel*, and *nearly two drachms of jalap*, and losing, at two periods, *sixty-two ounces of blood*, in three days! "After the (first) venæsection," says he, "which I performed myself, there being no surgeon present, *I found remarkable relief, the head-ach and anxiety were considerably alleviated.*" The second time, it also "*gave great relief.*" This

gentleman, after describing farther prodigies of blood-letting, goes on to observe : “ In short, the practice I found successful was San Grado’s ;” (p. 383.) He seems to have entirely overlooked the circumstance, that the original San Grado, having an utter abhorrence of chemical remedies, would not have degraded his two specifics, or ruined the uniformity of his practice, by the addition of such an incompatible remedy as forty-five grains of calomel.

Surely, if blood-letting were efficacious in any one species of pestilence, its utility could not fail to have been confirmed beyond question, in the course of the thousands of years, during which it has been practised, in so many various degrees. But we find that the advocates of this practice, as applied to diseases in general, often differ diametrically from each other, in respect to its advantages or disadvantages, in particular maladies. Thus, Mr. Pym is of opinion, that the fever of Gibraltar, or what he calls the Bulam fever, “ does not bear bleeding.” To which Dr. Burnet replies :—“ Now, this I totally deny, and *affirm that it not only bears, but requires this evacuation for its cure ; and that the practice of blood-letting and cathartics has had a success in the treatment of this fever, infinitely superior to every other.*” (P. 398.) And these positions he endeavours, to strengthen, by the testi-

mony of other practitioners, as Mr. Humphries, Dr. Macmullin, the surgeons of the 67th and 26th regiments, &c. (P. 399, 400.) On these wide differences of opinion, it may be remarked, that whilst the practice continues of receiving tradition and testimony, in proof of medical conclusions, it is quite impossible, that disputes respecting the treatment of diseases, should have an end. Relinquishing, then, such vain and frivolous attempts at probation, and abiding by the only fit mode of proceeding, in such researches, let us try, by induction of experiment, whether we cannot arrive at the conclusion, that, without blood-letting, purging, or any other mode of evacuation, diseases may be cured, in a certain time, and in a determinate manner, by the due and continued action of appropriate exciting powers ¹.

The specimens, which I have given, afford a sufficiently correct representation of the ordinary

¹ Who thinks of adducing testimonial evidence, in proof of any results in chemistry, or mechanics? The processes are repeated by various persons, and the truth, or error, of the alledged conclusions readily ascertained. Nor is it uncommon for investigators, in these sciences, to be the first to detect, and to divulge their own mistakes. This is magnanimity, and a proper deference for science. But, in medicine, it seems to be regarded as necessary to consistency of character, not to be less in error to-day, than you was yesterday.

treatment, as far as any has been applied, and of its results, in those epidemics, which have prevailed in any considerable degree of intensity; and, when we read dilated accounts of cures performed, in pestilences of inferior degree, as the epidemics of Gibraltar, in 1813 and 1814, already referred to, under similar modes of treatment, modes which we know, both from reason and experience, to be inefficient, the only conclusion that we can consistently form, is, that these reputed cures are merely recoveries, which have taken place, only because the disease had not been originally of sufficient intensity to prove fatal, and which would have equally happened, and often much sooner, had no medical treatment whatever been employed. The great importance of the subject has led me to enter more fully upon its merits, than may, by some readers, be thought necessary; and even to hazard occasional repetitions, rather than run the risk of leaving my meaning but imperfectly understood.

To conclude, I repeat, with the increased confidence derived from upwards of twenty years additional experience, (*experiment, observation, and inference, not* implicit conformity with an unquestioned routine) that blood-letting must always, and in every degree, be injurious, and ought therefore never to be performed. "Whatever has been useful in one case, must be useful

in every similar case of disease ; but blood-letting has not *invariably* been found useful in any *one* disease. We may therefore, I think, fairly conclude, that it has never been useful in any *one case* of disease. If it be said that this is reasoning, and that experience, let me be permitted to ask, whether just reasoning, and real experience, can ever differ ? It is impossible. Whatever is true in theory, must be right in practice. To inculcate the contrary opinion is the grand shield of empiricism. Circumstances, delivered as facts, from the presumed experience of individuals, ought never to weigh against principles, which are deduced from numerous and undoubted facts, and which can be put to the test of experiment by all mankind¹.”

¹ A View of the Science of Life, Bengal edition, 1796, p. 70, 71.

CHAP. XLVIII.

Removal of the sick into pure air, the first part of the treatment—changes recommended from one part of a town to another—other parts of treatment—the proper cure of plague deduced from experience in other epidemics—all conclusions in medical science founded on induction of experiment; and easily capable of being verified, or disproved—organs principally affected in epidemic diseases—the circulating system, and the brain and nerves—appropriate remedies—the manner of their application.

WHATEVER be the powers, which constitute the cause of disease, upon a knowledge of which depend the means of their prevention, the cause itself being always the same, *i. e.* a deficient excitation of the organs affected, the cure will consist in increasing, to a due degree, the excitation of these organs. A knowledge of the powers, which constitute the cause, is not indispensable to the cure. Thus, those morbid affections of organs, which constitute plague, are equally capable of being removed by a duly increased excitation of those organs, whether we consider the air, or contagion, to be the power, which constitutes the cause of that malady.

The cure, then, can be performed, *i. e.* the excitation of the organs affected can be raised by

appropriate agents, although the power constituting the cause should continue to act. Hence, the removal of the sick, from the air, which has produced the disease, into an air more pure, is not an indispensable part of the treatment: nor will it, of itself, be sufficient to effect a cure of epidemic diseases of the higher degree. Yet, as such removal will place the patients in a situation, to be sufficiently acted upon by a lesser degree of other curative means, it ought always, when practicable, to be the incipient step of the treatment. And this will be of great use, if the removal should be only from one part of a great town to another. In London, for instance, during the plague of 1665, a sick person would, in the early part of the season, have had his condition considerably improved, by being sent from St. Giles's to the city; and, at a subsequent period, perhaps by being sent from the city to St. Giles's; as the noxiousness of the atmosphere might have varied in those parts respectively. But it often happens, that the malady first manifests itself, when the person affected is situated in one place, although the cause should have been applied in another¹.

¹ During some of the epidemics of England, it happened, that, British subjects, to whom the cause of the malady had been applied before their departure, were afterwards seized, and died of it in foreign countries; which gave rise to the

Thus, he might be seized in the city, the air being yet good, although the cause of the disease had been applied at St. Giles's, Whitechapel, or Wapping. In that case, removal would be unnecessary, and might be prejudicial, unless it were to the purer air of the country. The change proposed, is, in other words, but the application of a remedy, or the appropriate increase of excitation, in the increased purity of the atmosphere.

The other means of cure, which I have, in practice, recently found to be efficient in plague, are the same, which had previously been deduced from experience of the results of the operation of remedies, in the fevers of Bengal, Batavia, Bencoolen, the yellow fever of the West Indies, remittents, intermittents, diarrhœa, dysentery ; and, in short, every acute disease, in which the circulating system is principally affected, or participates.

As all diseases are produced, so are they to be cured in a similar manner¹. It is an established rule, that the selection of the means employed should depend upon the nature of the organs

ridiculous conceit, that those Englishmen, who were abroad, and out of the reach of the cause operating at home, were notwithstanding instinctively followed and attacked by the malady!

¹ This proposition is explained at large, in my "Elements of Medicine."

affected. The first consideration, then, respecting the treatment of any disease, is, what are the organs more particularly affected? In the higher degrees of epidemic diseases, the organs affected are usually very numerous. From the direct and powerful operation of their cause upon the blood, and the constant circulation and action of that fluid upon all the parts of the living body, there are no other maladies capable of being so generally extended over the system. Hence, the almost infinite variety of phænomena, or symptoms, giving the delusive appearance, in severe pestilences, of so many various maladies; and the epithets, “proteian,” and “multifarious,” so commonly bestowed upon the plague.

According to the structure of the organs, which are, in each case, more particularly affected, the details of the treatment may admit of some considerable variation. Thus, beside the organs more immediately essential to life, the glands, the skin, the membranes, &c. may be diseased, in a great, or almost infinite variety of combinations, proportions, and degrees. But to adapt the means, to be employed, to each affection, of each organ, would lead to a complexity of treatment, which, if it were practicable, would be unnecessary. By overcoming the main body of the affection, we ensure the life, and the recovery of the patient.

It is sufficient, then, and to this our attention ought to be exclusively directed, to apply such means of cure as are capable of removing the morbid state of those parts of the body, which are more immediately indispensable to life. In epidemic maladies, these parts are the circulating system, and the brain and nerves.

What are the exciting powers, then, which act preferably upon the circulating system, becomes, here, the first object of consideration. Of these, the materia medica affords many: and it was my intention, had the opportunity been afforded, in the Levant, which might reasonably have been expected, to have, by a comparative trial, upon an extensive scale of experiment, of the principal known agents of this description, endeavoured to ascertain their relative degrees of utility, and of fitness. It soon, however, became obvious, that I must relinquish views so comprehensive; and be content with endeavouring to snatch the opportunity of verifying, in respect to plague, the efficiency of those agents, which I had previously applied successfully, in other epidemic diseases, and with the results of whose operation I was already familiar.

The exciting power, which I had been accustomed most frequently to employ, in diseases of

the circulating system, was mercury¹; and, in those of the brain and nerves, opium; and, in cases, in which these affections were combined, a combination of both these remedies.

Whether either, or both the remedies, and in what proportions, or combinations, they ought to be employed, will depend upon whether the circulating system only, or the brain and nerves

¹ The efficiency of this remedy, administered as an exciting power, as a cure for the plague was inferred, in 1796, in the following terms: "As, in the cure of those high degrees of exhaustion, (it should have been 'those *low degrees of excitement*,') constituting dysentery and fevers, mercury has been found to be one of the most useful remedies: and as plague is a disease depending also upon a very high state of exhaustion, (it should have been, 'consisting also of a very *low degree of excitement*,') *it is inferred that mercury will be found proportionally useful in the cure, even of that pestilential and fatal disorder.*"—A Treatise on the Action of Mercury, p. 34.

In another place, the same inference was stated, in the form of a question; and I may here observe, that its confirmation will be found to constitute a striking practical illustration of the difference between theory and hypothesis: "If eight grains of calomel, and four grains of opium, repeated every two, three, or four hours, will cure a fever, or a dysentery, of a certain degree, will not the same medicines produce the same effect, in plague, if given in quantities proportionate to the force of the disease?"—A Dissertation on the Source of Epidemic and Pestilential Diseases, Calcutta, 1796, p. 46.

only, or both, are affected, and in what degree respectively. It seldom, however, or perhaps never happens, that the brain and nerves are affected, without the circulating system fully participating in the malady, although there is reason to believe that the contrary is sometimes the case. But the disease appears far more frequently, or almost always as a combined affection, in which that of the circulating system generally preponderates.

For the most ordinary combination, then, and supposing it a mean affection, I would prescribe six grains of calomel, and two grains of opium, every two hours, day and night, until the disease was overcome; which I should expect to happen, under a due regularity in the administration of the remedies, within forty-eight hours, if the treatment was commenced, on the first, or second day, of the attack.

No inferences, unfavourable to these results, can be fairly deduced from the circumstances attending my patients at the Hospital, near the Seven Towers; for, besides, that none of them came under my care on the first, or second day, of the disorder, there was no possibility, as stated in my Report to Sir Robert Liston, of enforcing night-prescription; for the want of which, even for one night, a patient, in any stage of the disease, may, and, in an advanced

stage, must be lost. To a patient, even during the first, or second day, with his malady still decidedly curable, it would, under the circumstances of administration, which existed at the Pest Hospital, have made the difference of life, or death, whether his admission took place in the morning, or in the evening. Persons admitted before ten o'clock in the morning, would, in the regular course of prescription, have taken six doses of medicine by bed-time; by which, notwithstanding the necessary suspension of the remedies during the night, he would, in all probability, be placed out of danger: whereas a patient, admitted, under similar circumstances, in the afternoon, or evening, and not having time to take more than one, two, or at the utmost, three doses of medicine, before the period of their discontinuance for the night, would, in all probability, ere morning, have his malady rendered mortal.

In the more severe pestilences, supposing the relative degrees of the affections of the different systems, or organs, to observe the usual proportions, it will, of course, be necessary, proportionally, to increase the force of each of the agents employed, beyond what was adapted to the mean degree of the disease: as, for instance, seven grains and a half of calomel, and two grains and a half of opium; nine grains of

calomel, and three grains of opium ; twelve grains of calomel and four grains of opium, for the three next higher degrees.

On the other hand, in those degrees of pestilence, below the mean state, the relative intensity of the affections of the different organs not varying, the quantities may be reduced to five, four, three, two, and even one grain, of the former agent, and one and two thirds, one and one third, one, two thirds, and one third, of a grain, of the latter, for the five next inferior degrees.

In general, I prefer the internal use of calomel, to the external employment of ointment, only because the quantity intended to be brought into efficient application can be more correctly apportioned. There may be cases, however, in which particular circumstances, as the immediate rejection of all matters by the stomach, may render the external application of this remedy preferable. Two drachms, three drachms, half an ounce, six drachms, and even an ounce, may be then necessary, and ought to be repeated, at similar intervals with the calomel, until the circumstances, which have rendered it preferable, have disappeared. It will be better, then, to return to the calomel and opium, in such proportions as the severity of the disease may indicate ; or, in order to obviate the pernicious consequences (the dimi-

nished excitement, or disease,) to be apprehended from the cessation of the action of the remedy, recourse may be had to the internal administration of opium, in doses of four, five, six, eight, ten, or twelve grains, according to the intensity of the action of the agent previously applied, every hour, excepting during the intervals of sleep. After the first twelve, or eighteen hours, the quantity may be gradually diminished, or other exciting powers, of equivalent action, substituted in its place. Ambulating blisters, and repeated enemas, will, in such cases, be excellent auxiliaries, or part substitutes. For the latter purpose, two ounces of castor oil, two ounces of glauher salts, and six ounces of warm water, may be considered as applicable to cases of medium intensity.

The disease being removed, which will certainly always happen as often as the force of the remedies is raised somewhat above its level, unless extensive organic lesion has already taken place, the next object will be to prevent the new diseases, if you will, or the deficient excitement, which would arise, from the abrupt cessation of the action of the remedies. The symptoms, by which they would, in this case, be indicated, are those which usually succeed the misapplication of mercury, as salivation, hæmorrhagy, ulcerations of the throat, fauces, gums, and tongue, &c., accord-

ing to circumstances. But the diminished excitement, which would take place, in consequence of the sudden diminution of a degree of the action of mercury, sufficient to cure an ordinary pestilence, would be denoted by hæmorrhagy. There will, therefore, be as much danger, from the sudden subduction of the remedies, as from the original disease, or even more; excepting when the subduction happens to be made at the precise period, at which they have restored the excitement to the healthy standard. Under such circumstances, there can be no danger of a new disease. But, as these circumstances are, in the present state of our knowledge, difficult to be determined, the safer plan is always to subduct gradually; or, when that cannot be done, as when the patient takes a disgust to the remedies, and refuses to take them, some other exciting powers of equivalent force should be substituted. In this case, as the danger principally arises from the sudden subduction of the calomel, and as the opium may be increased with safety, the sum of the former that is withdrawn may be compensated for by a proportional increase of that of the latter. Whether the sum of remedial power has been too rapidly decreased, may be known, by the occurrence of symptoms indicating a fresh diminution of excitement, or a new disease. By the judicious use of ambulating blisters, repeated

enemas, and various other modes of excitation, substituted for those withdrawn, the excitement may be maintained at the due degree, until, by the addition of food, exercise, and the usual avocations, and enjoyments of life, health may be sustained, by the mere operation of the ordinary exciting powers. These results, I pronounce, from experience and observation, of the usual succession of phænomena, may, at all times, and invariably, be produced, excepting when there has previously existed an incurable lesion of one or more of the organs essential to life. It is quite sufficient for our purpose, that those agents and those modes of proceeding, with which we are already acquainted, are capable of ensuring the restoration of health, from the most dangerous maladies. That there are other and better modes, I have no doubt: that those, which I have already indicated, admit of considerable improvements, I am certain. That I should not have been allowed the opportunities of making my experiments upon the scale which I had projected, and of ascertaining, with farther precision, the most appropriate exciting powers, singly, in combination, or as substitutes for one another, which, under all the usual varieties of plague, could be brought, with advantage, into action, is to me, as it must, I should think, be to most others, a source of sincere and lasting regret.

But, I have the consolation to feel assured, that I shall appear to have done as much as was practicable, under circumstances so hostile to enquiry : and it would not be of the essence of justice, I should think, to object to me, that, because I had made a few more steps than my neighbours, in the right path, I should not have made still farther progress.

When any considerable deviation takes place, from the usual relative proportions of the affections of the circulating system, and of the brain and nerves, conformable variations in the treatment will be required. Thus, in proportion as the affections of the former is relatively greater than usual, the relative force of the mercury must be augmented ; and, when that of the latter, the relative force of the opium.

Some of the other systems, or organs of the body, participating largely in the disease, may require some variation, or additional combination, of the exciting powers. For instance, digitalis, hemlock, powder of litta, camphor, &c. may severally be combined with the two, which I have principally employed, or with each other, or substituted for one of them, according to the nature of the case. These suggestions are, however, at present, only speculative ; the opportunity of putting them to the test of experiment, having been unluckily wrested from my hands.

Perhaps, after all, this would be but a mere refinement; since, as the affections of the subordinate organs almost always follow the fate of those of superior importance, the excitement of the circulating system, and of the brain and nerves being restored to the healthy standard, no considerable detriment would often arise from the affections of parts of lesser consequence. To be solicitous about these minor considerations is probably therefore altogether superfluous; although they may be said to form, at present, the principal object of the practitioner, in the treatment, as far as prejudice admits of any, of pestilential diseases.

There is a remedy, which I have been in the habit of using, both for myself, and other persons, for upwards of twenty years, and has, of late, become fashionable in this country, of which I think it right to speak here; having employed it during, as well as before, my attack of the plague, at Constantinople: I mean the nitric bath. It was first suggested to me by the perusal of some ingenious essays, published in the journals of India, by Dr. Helenus Scott, of Bombay. If I recollect right, the nitric acid was directed, by Dr. Scott, to be taken internally. But I have found it both more convenient, and more agreeable, as well as equally efficient, in the form of a pediluvium, or foot-bath. In common

colds, the mean degree is two drachms in a pail of warm water, containing about four gallons; which may be augmented to three drachms, half an ounce, six drachms, and an ounce. In the plague, I sometimes employed, in my own case, from one to two ounces at a time; supporting its action afterwards by other remedies. The water should be as hot as the person can comfortably bear it; and the time a quarter of an hour.

Of cold affusion, oil frictions, and many other means, which have, at various periods, been recommended, I shall only observe, that, although, in minor degrees of disease, they cannot fail to be of a certain degree of utility; yet, when means decidedly efficacious are within our reach, it would be idle to trust to those, which are of a more uncertain, or a less efficient nature. Besides, in a matter of such extreme importance, it would be unavailing to speak of agents, which it would be impossible, with sufficient method, to apply, or of which we have not experimentally ascertained the precise value.

Beside the removal into pure air, and the application of appropriate exciting powers, there is a third indication of cure, applicable to the present condition of Christendom, which consists in obviating the adventitious causes of pestilence, constituted by the consequences of the erroneous

belief in contagion. Whatever dooms the body to inactivity, the mind to scenes of monotonous seclusion, and the lungs to the continued respiration of the noxious air, which at first produced the malady, must multiply, prodigiously, the otherwise inevitable calamities of pestilence. But, on this part of the subject, enough has been said in another place.

BOOK XI.

OF THE CAUSE AND PREVENTION OF EPIDEMIC DISEASES.

CHAP. XLIX.

OF THE CAUSE OF EPIDEMIC DISEASES.

THE cause of epidemic and pestilential, in common with all other diseases, is an undue, or diminished action of certain exciting powers, upon the organs particularly affected. The principal exciting power, is, in this case, the atmospheric air.

If the cause of any effect, be that, without which it cannot be produced; and if epidemic diseases never arise, but in a noxious atmosphere; it follows that a noxious atmosphere must be deemed the cause of these maladies. There can nothing else be necessary. Whatever be the precise nature of the changes in the properties of the air, by which epidemic affections are produced, as all diseases are the result of diminished

excitation¹, it is competent to infer, that, in this instance, the effect is produced, entirely in consequence of a diminution of the exciting power of the vital element. This change may consist of a diminished exciting power of the air, in its ordinary state, or when that power has previously been unusually augmented. Hence, in the production of pestilences, an importance is justly attributed to unusual vicissitudes of the atmosphere. “ From every record of epidemic and pestilential diseases, it appears, that they have their stated seasons of recurrence ; that these seasons are such months, as are most remarkable for vicissitudes of the atmosphere ; that they become general, only in years in which these vicissitudes are extreme ; that they do not occur in seasons, when the degrees of heat, or of cold, however intense, are equable, nor in years when the state of the atmosphere remains tempered throughout ; and that they uniformly cease, with the establishment of an equable state of the atmosphere, *whether the weather be hot, or cold*².” When I published these remarks, nearly twenty-two years ago, I was not aware that they could be supported by the authority of so philosophic

¹ In my “ Elements of Medicine,” it is shewn, that all diseases arise in the same manner.

² Dissertation on the Source of Epidemic and Pestilential Diseases, 1796, p. 21.

an observer as Dr. Arbuthnot, who affirms, that there are “ no changes in human bodies known, but are producible by the contents, properties, and qualities, which we are sure the air is endued with, and especially by their great enormities, and sudden successions, and alterations¹.” These remarks obviously point at similar doctrines, with those, which are here set forth; and it is presumable, that, if prejudice in favour of contagion had not, at the moment, run so strong, this ingenious physician would have expressed himself still more decidedly. I have already shewn, that the doctrines of Hippocrates, respecting the cause of epidemic diseases, if they have not *quite* embraced the truth, were a much nearer approximation to it, than those of any writer of succeeding times. I have refuted the gross and pernicious error, of the 16th century, of which he could not possibly have had any idea; and, with a copiousness of proof, which *he* would have deemed unnecessary, I now shew, that all general diseases, *i. e.* febrile affections, that are not symptomatic; excepting those which are acknowledged by all the world to be contagious, are always occasioned by noxious states, or extreme vicissitudes of the atmosphere. These causes may act separately, or combined: for, the state of the air

¹ Arbuthnot on Air, p. 174, 187.

may be at once noxious, and its vicissitudes extreme. But either of them will be fully sufficient, without the other, to produce a pestilence.

In Malta, in 1813, according to the diary of Mr. Corner, already quoted, the difference of *temperature*, by Fahrenheit's thermometer, in the night and in the day, was sometimes from 15 to 18 degrees; and was the greatest, during the most fatal period of the pestilence.

"The plague," says Dr. Whyte, "has ever been most prevalent, where the difference between noon, and midnight temperature, has been greatest; and at the seasons of sudden change from dry to moist, and from warm to cold. In no country is there, at some seasons, a greater difference between the temperature of day and night, than in Egypt. I am informed, by an English gentleman, who accompanied the Grand Vizier, in his late unfortunate expedition, that, although the Egyptian days were insufferably hot, the nights were so extremely cold, that he required three, and sometimes four blankets, to keep himself warm; and that the dews were so copious, as to penetrate the thickest canvass of the best pitched tent¹."

¹ From unpublished manuscripts of Dr. Whyte, which were communicated to me, as has been already stated, in October last. Having no space for them in this volume, it is my intention, in order that no injustice may be done to

That epidemic diseases are produced by vicissitudes, is demonstrable, both from the manner in which diseases in general arise ; and that, in which they are alone capable of being cured. That they may also be produced by the noxious qualities of the air, no one, I believe, will deny who understands the connection of air with life, and is not infatuated by the doctrine of contagion ; and *à fortiori*, that they may be produced by both.

If you were to expose the lungs of a whole community, *seriatim*, to an air that is noxious in various degrees, as that of the Grotto del Cani, and many other places, having been previously prepared by admixture, in different proportions, with pure air, will any one expect, from such an experiment, any other result, than the production of a disease, which, in its nature, phænomena, and cause, is, in every respect, a pestilence. In jails, hospitals, camps, and ships, or, in general, in all situations of confinement, in crowds, similar effects are produced, in the precise degree, in which the air becomes vitiated. A typhus may be produced, in the course of a few days, by shutting down the hatches of a ship, in bad weather, if there be many people breathing between

the memory of this worthy man, and intrepid investigator, to transmit them for publication, as a whole, to the Medical Journals.

decks. Howard affirms that the jail fever is unknown in the prisons abroad; and he justly concludes, that, as the cells and dungeons are as offensive and dirty there as in England, there must be some other cause than want of cleanliness for its production¹.

Another writer assuming that jail fever is *incapable* of occurring in the prisons abroad, straight constructs an hypothesis respecting its cause. "This *additional* cause," says he, "*can be no other than the contagion thereof*²." Here we have the extraordinary phænomenon of a contagion *rooted to the soil*! Let us estimate these assertions at their just value. Typhus, or jail fever, is *said* not to exist in countries that are *warmer* than England, as Venice, Naples, &c.³; nor, in countries that are *colder* than England, as Russia, &c.⁴; yet we are told, that this disease is rendered more virulent, and morbidic, by severe *frost*⁵!

After the elucidation, which has been given, of the real cause of jail fever, it must be obvious, that mere degrees of heat, or of cold, cannot, more than contagion, have any thing to do with the matter. Admitting the fact to be correct, that jail fever is not often to be met with in foreign countries, it demands a very different explanation.

¹ Lazarettoes, p. 131.

² Bancroft's Essay, p. 150.

³ Id. p. 146, 7.

⁴ Id. p. 149.

⁵ Id. p. 143.

I fear we shall be obliged to account for it, in a manner, although, in a view of science, more satisfactory to our minds, as inhabitants of Britain, much less so to our feelings. The jails in foreign countries are never so crowded as they are in this kingdom. The number confined in them is generally small; and the mere walls of a prison, with a turnkey, and one or two prisoners, will never, even with all the filth imaginable, be sufficient to produce a typhus. It is the *cause* of jail fever, *viz.* a vitiated state of the air, in prisons, occasioned by the breathing of a crowd of people, that is wanting, in foreign countries; not the *capability of its arising*. It seems, indeed, extraordinary, that circumstances so obvious could have been so long, and so decidedly overlooked. It could only have arisen from the manner in which the subject has been perplexed, and the confusion of ideas necessarily prevailing, that so much importance could have been attached to the want of cleanliness, in producing disease. Thus, the exemption of Persia from plague, which must depend upon the situation of the country¹, and upon local circumstances, has been

¹ The whole of Persia is situated in latitudes, that may be considered, from mere geographical position, very favourable to health. The capital is in a parallel that is reckoned to be the very first in point of salubrity. Many local circumstances, no doubt, also concur, to produce this happy

attributed to the cleanliness of the Persians; whilst the frequency of pestilence in Turkey, has been attributed to the want of cleanliness of the Turks, although they are perhaps the most remarkable for that virtue of the two people. This would have been a wrong explanation of the matter, even if the plague had depended upon contagion. The small-pox will not sooner affect the dirtiest, than the cleanest child. If either want of cleanliness, or contagion, or mere heat or cold, had any thing to do with typhus, or jail fever, that disease would occur much more frequently and severely in foreign countries, than in England. But, although the qualities of the air of each country, and consequently of its jails, may be modified by circumstances of moisture, qualities of soil, vicissitudes of temperature, &c. and the diseases produced by the air may be aggravated by the adventitious causes of mortality, resulting from the belief in contagion, &c. they would occur, *cæteris paribus*, in every country alike, in which the atmosphere is vitiated by the same means. These are, *in prisons, the breaths of prisoners.*

exemption from pestilence. The direction of the hills, and of the winds, the nature of the exposure, of the soil, and minerals, together with the masses of stagnant waters, &c. by which various countries are distinguished, must determine, other things being equal, their degrees of general salubrity.

But, where not a sufficient number exist to produce this vitiation, which constitutes the cause, jail-fever will, of course, not arise. Morally, imprisonment must be more distressing to Englishmen, than to foreigners, who are less accustomed to freedom. The vicissitude is greater; and this would prove an aggravation of disease.

Another cause of aggravation is common to Christians of all nations; the dread arising from the belief in contagion, which, under the existence of jail fever, must always be presumed to operate with great force¹. From this source of mortality, the Turkish prisoners, if their jails were sufficiently crowded to produce fever, would be exempt. The modification of epidemic or pestilence, called scurvy, is determined, together

¹ In September, 1815, an alarm of a contagious fever was spread in the King's Bench prison, in Southwark, by a medical man, who was confined there. Sir Lucas Pepys, and Mr. Dixon, Surgeon, of Newington, who officially attended the prison, were directed, by the government, to enquire into the matter. They reported the disease not to be contagious. But the consternation that prevailed, in consequence of the rumour, was such, as to deter many of the friends of the unfortunate persons in confinement, from visiting them, and to occasion the utmost inconvenience and distress: and, if the disease and the rumour had gone on increasing, the consequences could not fail to have been calamitous in the extreme.

with deficient nourishment, confinement, and the other causes of moral depression, operating in the course of long voyages, to a deleterious air occurring at sea. It is to be met with in India, as well as at Kamschatka, and I will venture to say, is capable of being produced any where at sea. Much ingenuity has been misemployed in searching for the cause of scurvy, as well as typhus, on board of particular ships, as if, together with what are called the occasional and predisposing causes, currents, or veins of foul air, should not be casually met with at sea, or in harbour, by individual ships, by which other ships, of the same fleet, or squadron, are not at all, or but slightly visited. "Some tracts of air offend the eyes, some the teeth, some are scorbutical, others dysenterical, others febriculous, arthritical, pestilential, &c.¹"

What kind of affection takes place, under the diminished exciting power of the air, must depend, beside the degree of diminution, upon the previous state of the organs. But when the degree is intense, hæmorrhagy has been known to be produced, in the same manner as from the sudden diminution, or subduction of the action of mercury, after having been previously applied in a high degree. It is related by Josephus Acosta,

¹ Loimotomia, London 1666, p. 19.

that, in ascending the hill of Pariacaca, in America, the effects of the air are violent vomitings, even to blood, defection of the spirits, and faintings. These are precisely the symptoms that would arise, upon the cessation of the action of large quantities of mercury, after having been previously, for some time, applied ; and without knowing the precise changes in the qualities of the air, that did, on this occasion, take place, we may safely infer it to have been accompanied by a diminution of its exciting power, by which the diseases, indicated by the phænomena mentioned, were produced in the travellers.

A distinct instance of the deleterious effects of a vitiated atmosphere, on the living body, is to be found in the jail fever, which proved fatal to the judges and audience, at the black assizes of Oxford, in 1577, which affected none but those who were present at the assizes, and manifested its effects in the shape of fever in some, and of diarrhœa in others.

When, in the early periods of the English power, in India, their small factory, at Calcutta, were forced to remain a whole night, in a crowded dungeon, that the slumbers of Surajah Doullah might not be interrupted, they almost all perished in the utmost agony¹, most of them in the course

¹ Howell's Narrative.

of the night. This affords an apt illustration of the manner in which persons may be speedily carried off by the plague, when its cause has been applied in considerable intensity.

The same principle is exemplified, in a lower degree, by the effects of respiring the foul air of a theatre, or crowded assembly, in producing head-achs, and other ailments, to which individuals may, for the time, be most liable, from the pre-existing state of their organs respectively.

The exhalations from a damp soil, as at burials, are apt to produce fevers, and a variety of forms of disease. This frequently happens, in some parts of Ireland and Scotland, where the relations of the deceased generally attend his funeral, in crowds; and are often placed, by intoxication, in a state more liable to be affected with disease. The malady which generally arises, under such circumstances, is fever; and, if the deceased has also happened to die of fever, that of the survivors never fails to be imputed to contagion.

The damp air of churches is also very apt to produce diseases. Mead relates that, at Marveges, more than sixty persons were suddenly seized with the plague, in a church, "into which a man had entered who had come out of an *infected* house." It is not impossible that the first part of this narrative may be true: but the conclusion intended to be inferred from the latter

part of it, is obviously absurd. The disease not being communicable, even according to the believers in contagion, beyond a few paces, one person could not have infected sixty, in the course of an ordinary sermon, unless he had gone round on purpose, to communicate it to them; which is not alledged to have been the case: besides, we are not informed whether the person in question was, himself, labouring under the disease, but only that he came out of an infected house!

The winds are capable of producing epidemic diseases, according to their degrees of impulsion, or the qualities of the bodies, over whose surfaces they blow. Thus, persons sitting exposed to currents of air, blowing through the crevices of windows, doors, chimneys, stair-cases, &c., are liable to various degrees of catarrh, rheumatism, fever, &c., according to the intensity, and extent of the currents, and the previous state of their constitutions, or organs respectively; whilst those, who are exposed to it more obliquely, and whose excitement is in a more vigorous state, will be more slightly affected; and those who are more distant, or almost entirely out of its reach, and in high health, will escape unhurt.

Winds, blowing over salt-petre grounds, as in the East Indies frequently happens, or merely being in contact with these grounds, will, according to circumstances, occasion fever, diarrhœa,

dysentery, spasms, or convulsions. The varieties of soil, in different countries, may serve, with other circumstances, to explain the difference in the forms of fevers.

Thus, fevers may arise conjointly from exposure to currents of air, blowing over salt-petre grounds, or soil containing matters calculated to impart various qualities to the air, and to the rays of the sun, perhaps combined, or alternated, with moisture, intoxication, or excessive fatigue, as frequently happens to soldiers, and sailors, especially in hot climates¹. These affections will assume appearances almost infinitely various, according as these causes happen to be modified, alternated, or combined. But they cannot differ from each other in kind: and the properties of the air have generally the greatest share in their production. The progress of deterioration of the atmosphere, is well exemplified in the extinction

¹ I have known a whole boat's crew, with the officer, who commanded them, seized with a fever, upon returning on board the ship to which they belonged, from burying the boatswain, ashore at Kedgerree, on the banks of the Hoogly. Here the cause was a mixed one. Exposure to the sun, and fatigue in rowing to and from the shore; exposure to noxious exhalations, in digging the grave; and very probably a certain degree of intoxication. The cause was severe; for they almost all died, except the officer. The sailors were treated according to the ordinary doctrines of the schools: the officer, who placed himself under my care, was treated according to the principles here set forth.

of light, and of life, in a cave of Upper Egypt, as described by a recent traveller ¹.

It has been observed, that in some parts of Spain, especially in Andalusia, fevers are more violent during the prevalence of an easterly wind, which is there called a *levanter*, or the wind of discord ². Towards the end of 1799, and before the epidemic of 1800, it was observed that the weather was remarkably severe. "The summers," says Mr. Amiel, "preceding the epidemics of 1804, 1810, and 1814, (at Gibraltar) have been chiefly remarkable for a long continuance of easterly winds." Burnet, p. 240. At Cadiz, the easterly winds, which generally precede epidemics, occasion very distressing feelings, even when their effects do not amount to disease.

The wind, blowing over marshes, is capable of producing also a variety of disease, but most generally occasions fevers of the intermittent form. An intermittent at Gibraltar, or the Cape of Good Hope, must be as rare an occurrence, as a *causus* or ardent fever, in the fens of Lincolnshire, or Essex. But neither moisture, nor heat, act upon the living body, but by the qualities, which they impart to the air. Marshes, as such, do not ne-

¹ Legh's Narrative, &c.

² Fellowes' Reports, pp. 13 and 15, and Townsend's Spain, p. 439.

cessarily produce intermittents ; nor heat, as such, causus ; otherwise the former would constantly exist, where there are marshes, and the latter, where there is heat. Dr. Pym and Sir James Fellowes were aware of the absurdity of the doctrines of Drs. Burnet and Bancroft, in imputing the fevers of the fleet, at Carthage, and of the garrison of Gibraltar, to Marsh Miasmata. Marsh Miasmata on ship board, and on a rock ! These are only the old doctrines of Lancisi and Cullen, served up in pejorated forms. Besides that fevers appear only *at the usual seasons*, where Marsh Miasmata *always* exist ; fevers appear at the same seasons, in other places, where there are no Marsh Miasmata. But, the cause always existing, they would never cease ; and, the cause not existing, they would not take place.

Drs. Burnet and Bancroft are no less clear-sighted, in perceiving the absurdities of Dr. Pym and Sir James Fellowes, in imputing the recent fevers of Gibraltar and Spain to contagion. Why could they not extend their reasoning to other epidemic diseases ? It seems as if they held it impossible, that there should be any other causes of fever in the world, than Marsh Miasmata, and contagion !

In order to preserve the appearance of consistency, the contagionists have felt themselves obliged, as we have seen, to admit, that *a certain*

constitution of the air is necessary, along with contagion, to produce epidemics. With this sophistry, Dr. Russel, and other modern contagionists, have first succeeded in deluding themselves, and afterwards in vain sought to delude others. It is a shallow attempt to compromise the dispute, to which science cannot yield a particle of assent. This admission is, in fact, a last shift ; and virtually an abandonment of the doctrine.

Beside the real causes of the misery and mortality incidental to pestilences, there are, in Christian communities, adventitious causes, which increase these calamities fourfold. These causes consist in the means, in consequence of the erroneous hypothesis of contagion, usually adopted in these communities, for their prevention, and in other effects of that belief. The institutions of police, which have sprung from this extraordinary opinion, frequently occasion the exposure of whole cities, by compulsion¹, to the constant operation of the proper causes of epidemic diseases, as noxious air ; but the belief creates various adventitious causes of mortality and misery, where such calamities prevail, as restraint by quarantine, lazarettos, &c. ; terror ; a deficient supply of provisions ; want of medical and other attend-

¹ Valletta, in Malta, and town of Noya, as described in this volume.

ance ; the abandonment of friends and relations ; death from absolute starvation ; and frequently even the murder of individuals called infected, in order to prevent the imaginary danger of their infecting others.

Upon the intensity with which both the proper and adventitious causes of pestilence are applied, depend, of course, both the severity, and mortality of each pestilence. These must consequently be as various, as the degrees of intensity, in which the causes are applied. To attempt any average scale of mortality, as has been usual with writers upon this subject, must therefore be obviously nugatory. The deaths may be from one in a hundred, to ninety-nine in a hundred. It is impossible to assign, *a priori*, any specific mortality to a pestilence, even under given circumstances, because the mortality itself can alone give the measure of the degree of severity, with which the causes are operating. I therefore reject all the observations, and alledged facts, upon this subject, of which I had collected a great number ; finding them, according to more correct notions of the subject, to be actually of no value.

CHAP. L.

OF THE PREVENTION OF EPIDEMIC DISEASES.

As epidemic diseases depend upon the qualities, or the vicissitudes of the atmosphere, in its capacity of an exciting power ; so, the proper means of prevention will consist in the general removal of the persons exposed to the operation of those qualities or vicissitudes, into air more pure, or more equable ; or, in situations, where, consistently with the views, occupations, or necessities of the inhabitants, such general removal cannot be effected, in maintaining the excitement of each individual, at the highest practical degree of vigour, so that he may be enabled to resist the influence of those qualities or vicissitudes of the atmosphere, to which he must continue to remain exposed.

Of the qualities of the air, upon which these maladies depend, we are ignorant, excepting as they manifest themselves in their effects ; and, if we had, at all times, the means of ascertaining, it would be but of little avail, unless we could have,

at the same time, the power of remedying them. All attempts to alter the properties of the great mass of the circumambient atmosphere, by such partial means as are in our power, must obviously be both absurd and nugatory. Both reason and experience concur to shew, that fumigations, explosions by gun-powder, firing of guns, kindling of large fires, and all expedients of a similar nature, which have been resorted to, on such occasions, must be inadequate to the object proposed, if not, upon the whole, injurious.

But the practice of confining persons to an atmosphere, in any considerable degree pestilential, as has been, and continues to be practised, under the baleful influence of the doctrine of contagion, without even a knowledge of the means, by which individuals might be enabled to resist its noxious operation, is almost certain destruction to communities. This fact is well illustrated by the recent proceedings, and consequent mortality, at the unfortunate town of Noya. The case of the inhabitants of this little town, as I am enabled to shew, from authentic official documents, forms a striking proof of the inhumanity and destructive effects of the regulations enjoined in consequence of the prevailing belief in contagion, and of their total inefficiency for stopping the disease. It commenced toward the end of November, 1815. In December, or early in Ja-

nuary, a *double ditch* had been dug round the town, and a *triple cordon* of troops had been placed¹. *The peasants in the neighbourhood*

¹ Copy of a communication upon the subject, from our Minister at Naples, to the Secretary of State:

Naples, 20th January, 1816.

“ My Lord,

“ I am concerned to state that very little doubt remains as to the nature of the disease at Noya.

“ It certainly appears to be the plague, though of a mild sort, the individuals attacked resisting it from twelve to twenty days. Every possible precaution has been taken to prevent it spreading; a *double ditch* has been dug round the town, and a *triple cordon* of troops has been placed. *The peasants in the neighbourhood are also armed*, and it is *therefore* nearly impossible, that this disease should be communicated farther.

“ I have the honour, &c.

(Signed) “ W. A'COURT.”

On the 16th of December, 1815, Mr. Hoppner, Consul-General at Venice, writes, “ that the plague had re-appeared in Dalmatia; and, that, in consequence, the whole of the coast had been again placed under the strictest quarantine.” As it cannot be doubted, that every town in Dalmatia, Istria, and along the coast of the Adriatic, which had the misfortune to be visited by this calamity, would suffer a similar treatment with Noya, it is afflicting to reflect upon the condition to which the miserable inhabitants of these countries must be reduced, in times of pestilence, by the scourge, twenty times more dreadful than the disease, of a cruel and unnecessary police.

were also armed, in order to repel, or to shoot any of the miserable inhabitants, who, in their despair, might endeavour clandestinely to escape from the focus of the malady. It appears, also, that internal means of separation, seclusion, and restriction, were enforced with similar rigour, in the interior of the place : for, in an hospital return, with which I was favoured, by Mr. Niven Ker, of the Levant Company, it is stated, that, on the 24th of March, there were 1135, or one-fifth of the inhabitants, in observation¹.

By this report it appears, that, in four months, more than one-tenth of the population perished : that the deaths were nearly double the recoveries : and, that, one-fifth of the inhabitants remained in a state of observation, or surveillance, requiring the services of almost as many more to watch them. Thus the community is deprived of the productive labour of nearly two-fifths of its number. This is a striking illustration, upon a small scale, of the destructive consequences of the belief in the extraordinary doctrine of contagion. To this source alone may be attributed more than three-fourths of the mortality, and

¹ Report from the Hospital at Noya, from the 23rd of November, 1815, to the 24th of March, 1816. Total deaths, 582—sick, 180—recovered, 374—in observation, 1135—population, 5700.

misery, which afflicted the town of Noya, upon this occasion. It is not probable, I think, that any pestilence ever existed, in which, in the absence of the adventitious causes of mortality, constituted by the effects of this disastrous belief, and the sick being left merely to the operation of the surrounding elements, more than one-half of them have perished in consequence of the intrinsic severity of the disease¹: and it is, by no means, probable, that the plague of Noya was within many degrees of being amongst the severest that ever existed. Mr. A'Court, indeed, declares it to have been, "of a mild sort, the individuals attacked resisting it from twelve to twenty days." Being well assured that no case of plague which the persons attacked can resist for twelve days, is, under an efficient mode of treatment, incurable, I feel myself justified, in concluding, that either under such mode of treatment, or in the absence of the consequences of the belief in contagion, the deaths at Noya, instead of 582, would not have amounted to fifty: and, that, under the advantages of both, they would not have exceeded twelve.

¹ Pestilences, occurring during sieges, as the plague of Athens, in which the constraint, imposed by the enemy, operates like the belief in contagion, are not comprised in this view of the subject.

The effects of the belief in contagion upon commerce have been fully explained in the first volume. I shall now farther illustrate them by an official communication, which passed, upon the occasion of the plague at Noya. It is addressed to the Commissioners of His Majesty's Customs.

Treasury Chambers, 7th Feb. 1816.

" Gentlemen,

" The Lords Commissioners of His Majesty's Treasury having received information that the plague has made its appearance at Noya, and along the coast of the Adriatic, and at Corfu, and in the territories contiguous to Fiume, and in the peninsula of Istria: I have received their Lordships commands to communicate this information to you, and to desire that you will give immediate directions that all vessels coming from the islands in the Mediterranean, or from the coast of Europe, within the Straits, should be treated in the same manner as vessels coming from Turkey, or from the coast of Africa, with suspected bills.

" I am, &c. &c.

" GEORGE HARRISON."

We find that the baneful consequences of the same error have also extended their influence to

the epidemic diseases of animals. The following will, I am persuaded, at some future period, form an interesting record, in the history of human opinions.

“ The Lords of the Privy Council, in a note, dated 7th February, 1816, desire James Buller to inform the Commissioners of His Majesty’s Customs, that a very alarming mortality prevails among the horned cattle in the neighbourhood of Calais and Boulogne, and that no horns, hides, or hoofs, either raw or salted, are to be admitted in any English ports, without it can be satisfactorily proved they were not slaughtered in any part of the districts of Calais or Boulogne.

“ JAMES BULLER.”
To George Delavaud, Esq.

It is not my intention to attempt any detailed account of the numerous means which have been usually employed for the prevention of epidemic diseases, farther than may be necessary to elucidate the general principles of this work. In as far as any thing certain, respecting their operation, can be relied upon, we find that the agents reputed to be most efficacious, are such as are of high intensity, as exciting powers. These, it can only be proper or necessary to employ, when, in pestilential seasons, persons cannot be removed

from the influence of that portion of the air, which is noxious: and they can only be applied to the individuals of the community in detail. In the narrative of my experiments, in the Levant, the good effects of brandy and water, as a drink, in the plague, has been stated: and what is good, during the disease, although not sufficient for the cure, may be reasonably presumed to be useful toward its prevention. Among the natives of the Levant, one of the prevailing precautions is the employment of small drams of the spirit called *rackey*, frequently repeated. The same practice, in respect to gin, (cline soapkies) is prevalent among the Dutch, in their unhealthy settlement of Batavia¹, in the East Indies, as well as at Walcheren, and in other parts of Holland, subject to epidemic diseases. In reference to the plague of London, in 1665, the virtues of sack have been eulogized by Dr. Hodges. And, of Diemberbroek, during the plague of Nimeguen, we are informed: “*Nec terrori, nec iræ, nec mærori, locum concedebat. Sed fatetur, se vino interdum indulsisse usque ad hilaritatem, ad ebrietatem nunquam*”². Oil

¹ It is the *town* of Batavia only that is unhealthy. The island of Java, *generally*, is remarkable for salubrity.

² Antonii Canestrini *Pestis Diagnosis, &c. Salisburgi, 1795*, p. 39.

frictions, as originally recommended by Mr. Baldwin, have been used, amongst other places, at Smyrna, by Fra. Louigi di Pavia, and at Odessa, by the order of the Duke de Richlieu, who directed it to be employed especially on the carriers of the dead. These persons generally escaped: and their exemption was attributed to the frictions.

I have already had occasion to remark, on the reports of persons being seized with plague, whilst said to be under the influence of mercury, and to shew that these persons were only under the influence of its misapplication. As mercury, administered in a proper manner, will cure; so, there cannot be a doubt, that, administered in a proper manner, it will prevent the plague. When in the Pest Hospital, I did not make any experiment of this kind upon myself, deeming it to be unnecessary. My apprehension of being attacked with the malady was not sufficiently great to induce me to adopt any serious measures with a view to prevention. The weather being hot, and my lassitude considerable, I used occasionally to sponge my body with cold water acidulated with nitric acid. But this was simply with a view of maintaining my sensations in a comfortable state, to enable me more cheerfully to sustain the fatigue, which it was necessary to undergo, in the situation in which I was

placed. This agency, to be sure, as far as it went, would act as a preventive. But, in this case, the quantity employed was not sufficient for such a purpose. Had prevention been my object, I should undoubtedly have employed mercury.

The common sense and experience of the bulk of mankind, have, in some measure, accorded with the principles of science, in establishing certain popular modes for the prevention of epidemic maladies. One, which has undoubtedly been found amongst the most efficient, is free living to a certain degree. To this practice, together with the absence of fear, it may, perhaps, be imputed, that British sailors, in the ports of Turkey, are not more, but, on the contrary, less subject to plague, than other descriptions of persons ¹.

¹ I have been assured, by Mr. Green, Treasurer of the Levant Company, a gentleman of accurate observation, who, during a residence of many years in Turkey, and in the course of an extensive commerce with that country, has had the very best opportunities of information, that instances have rarely, if ever, occurred, of English sailors, belonging to the merchant vessels, trading to that part of the world, being seized with the plague, and dying of it. But, if this disease were capable of being propagated, either by persons, or goods, no class of people upon earth would be more liable to be affected, than persons, who are almost perpetually exposing their persons to every risk of existing contagion,

by frequenting the lowest and most suspicious quarters, of the towns which they visit, and almost daily employed in hoisting on board, and stowing, and unstowing bales of goods, of those kinds which are reputed the most susceptible of infection. Persons so occupied could not possibly be long exempt from a malady, which was capable of being thus propagated. Mr. Green, and other persons of experience with whom I have conversed, without entering minutely into the merits of the question of contagion as a whole, could never be led to believe that plague can be communicated through the medium of merchandise. The fact, which I have mentioned, respecting the exemption of sailors, is admitted by Dr. Russell; but attempted to be explained by him, in a manner conformable to the doctrine of contagion, which he had adopted. It is true, that several of the sailors, employed on the British expedition to Egypt, were seized with this malady, in the autumn of 1801, on board of some of the men of war and transports in Aboukir Bay. But, on none of these occasions, was the proportion of the crews seized greater, than generally happens, on similar occasions, as in watering, or other hard duty, in deleterious climates. The individuals only, who were employed on those services, or exposed to the sun, at hard labour on board, or in rowing boats to considerable distances, or in sleeping in improper places ashore, or had drank immoderately of spirituous liquors, &c. during the pestilential season, were so affected. The disease did not, in any case, extend to the crews generally. I have, in a preceding chapter, stated an instance, related in the papers of Dr. Whyte, in which free communication took place, between a pestiferous sailor, and the rest of the ship's company, and the disease did not spread.

CHAP. LI.

Sketch of the origin and progress of the doctrines promulgated in this work—my experiments on mercury commenced in 1788, and brought to considerable maturity in 1796—my doctrines concerning epidemic diseases published that year—propose to publish a collection of reports of cases, of twenty years old, treated according to principle, by several practitioners—diffused circulation of my doctrines in Asia, Europe, and America—their extraordinary reception and treatment in Britain—Dr. Dick introduces the use of mercury in acute diseases into London—conduct of the recent authors upon the epidemics of Spain.

As, when these doctrines shall have been fully established, some curiosity may possibly be manifested to know their origin and progress, and as the tracts, in which they were first promulgated, have been but partially published in Europe, I think it right to give some account of them in this place. Such a statement may also tend, if not to deter from farther disgraceful attempts at plagiarism, at least to frustrate even their temporary success; as well as to render any farther notice of them unnecessary on my part.

In 1788, during a voyage to India, I had begun to use mercury, *as an exciting power*, for the cure of diseases.

In January 1789, I made the first decisive experiment, with this agent, in intermittent fever. It was in my own person, and proved completely successful¹. The result was so satisfactory, that I resolved to continue the practice, in future, in every case of this disease. Since similar processes, under similar circumstances, must always produce similar results, I concluded, unless there were some fallacy in this case, that mercury would prove a certain cure for intermittent fevers; my expectations were not disappointed.

In the year 1790, in the island of Jamaica, I extended the same treatment to yellow fever², as well as to gout and bleeding discharges³, with benefits no less decisive.

In the years 1791 and 1792, in the course of a voyage to the East Indies, I employed this agent with unequivocal success, in jaundice, ophthalmia, and hectic fever⁴.

In the year 1793, I ascertained, in the course of treating a case of coup-de-soleil, in my own person, at Calcutta, in Bengal, that the diseases which succeed the application of mercury, indicated by salivation, hæmorrhagy, and a variety of other symptoms, are not the result of its imme-

¹ A Treatise on the Action of Mercury upon living Bodies. Calcutta, 1796, p. 6.

² Id. p. 8.

³ Id. p. 13.

⁴ Id. p. 16.

diate action; but of the diminution, suspension, or cessation of its action ¹.

In 1794, during a twelve month's cruise in the eastern seas, I had an opportunity of ascertaining its efficiency in diarrhœa, dysentery, and that species of fever, usually called typhus, which is indigenous of Batavia ².

In the year 1796, in the worst ward, in the Calcutta general hospital, in all India, and probably in the whole world, called the Mariner's Ward, my researches, concerning the action of mercury upon the living body, alone, and in combination, or alternated with opium, and other

¹ A Treatise on the Action of Mercury upon living Bodies. Calcutta, 1796, p. 23.—“Ulcerations of the throat, soreness of the mouth, salivation, purging, strangury, costiveness, &c. arise, not from the immediate action of mercury, but from its irregular application, or sudden subduction. If any one affects to doubt this fact, let him take one grain of calomel every hour, for twenty or thirty hours, and then stop. He will find that his mouth does not become sore while he is taking the calomel at regular periods, but some hours after having left it off; that the soreness will continue to increase for some time after having desisted from taking the medicine; and may be diminished or removed, by a proper re-application of the same power.”² Treatise, p. 33. Here we find all the hypotheses, respecting the action of mercury upon the living body, which have been framed during the thousands of years that it has been in use, as a remedy, overthrown by a single experiment.

² Id. p. 25—27.

exciting powers, were brought to still greater maturity. In other wards of that hospital, a similar treatment was, at the same time, pursued, by my friends, Dr. William Yates, of the Madras, and Dr. James Robertson, of the Bengal establishment; and with similar beneficial results.

The patients in the Mariner's Ward, consisted of worn-out soldiers, from the interior of the country, who had resided many years in India; of sailors from long voyages in the coasting trade; and of townsmen, who had previously, without benefit, run the gauntlet of almost all the resident practitioners of the presidency. They were generally in the last stages of hepatitis, dysentery, fever, peripneumony, or dropsy; and many of them with complications of several disorders. Whilst those in the other wards consisted exclusively of young men, from the European corps, in garrison, in the neighbourhood, seldom labouring under diseases severer in degree, than gonorrhœa, or slight intermittent. Soldiers are invariably sent to hospitals, in the incipient stages of sickness, and before their diseases can have become formidable. But the patients destined for the ward under my care, were all in a dangerous state, or incurable, at their entrance. It seemed to be a sort of condemned ward. Yet, under such dissimilar circumstances, were attempts made, by a comparison of relative mortality, to

deduce inferences, and to propagate reports, unfavourable to my method of treatment ! The only refutation, which I could then offer, was to propose that my opponents should take patient for patient, under similar circumstances, of constitution and disease ; confident of an easy victory, if they would abstain from applying the very means, which they were so strenuous to condemn. This proposition could only be evaded, by pretending a reluctance to try experiments with the lives of men ; as if it were not manifest, that *my* experiments, *which were always first tried upon myself*, were capable of being conducted with perfect safety ; or, as if the practice of medicine, in its conjectural state, were any thing else, than *a continued series of experiments, upon the lives of our fellow-creatures.*

In August 1796, some account of my experiments, and inductions, upon that occasion, as well as of former researches, were published, in three distinct tracts, at Calcutta, in Bengal, as has been already mentioned. Amongst these, was the Dissertation on the Source of Epidemic and Pestilential Diseases, which constituted the original outline of the work now finished. These publications procured me the distinction of being heartily reviled, by those, who considered themselves as medical authorities in the east ; who, instead of attempting to refute my arguments,

discreetly contented themselves with representing me as a madman, and calling me names. The most easy and obvious mode, at all times, of getting quit of doctrines, which it is predetermined not to receive, but which it is found impracticable to refute, is, a cautious silence being observed respecting their merits, by assailing their author with

Slander,

Whose edge is sharper than the sword ; whose tongue
 Outvenoms all the worms of Nile ; whose breath
 Rides in the posting winds, and doth belye
 All corners of the world.

But, notwithstanding the torrent of medical authority, which they had, in the first instance, to encounter, it is but justice to declare, that one half of my colleagues at the Calcutta general Hospital, immediately embraced my doctrines, not by acclamation, but by the light of conviction ; and, as far as they were not restrained, applied them to practice. Nor should I omit to state, to the credit of Dr. Francis Balfour, who presided over that institution, at the time of prosecuting my researches, that he shewed himself, under circumstances of no ordinary delicacy and difficulty, an open, fair, and generous adversary. In afterwards refusing to concur in certain views of mine, to the success of which he might have contributed in Europe, I feel a plea-

sure in recollecting that this ingenuous physician had the nobleness and candour to address his objections directly to myself. And, whatever might have been their merits otherwise, as they were undoubtedly founded upon principle, I have never ceased to entertain for this amiable gentleman, the same degree of respect, as if he had contributed, with all his might, to the propagation of those medical principles, which alone I hold to be correct¹.

¹ It is remarkable, that, between 1796 and 1805, a considerable, although not a complete, change of opinion had taken place, in the mind of Dr. Balfour, respecting the treatment of fever. He, who, in 1796, confides almost exclusively in emetics, and bark, and objects *toto cœlo* to the use of mercury, in 1805, begins to entertain very favourable notions of that remedy: "Considering that obstructions in the liver very frequently shew themselves, in the common fevers of this country, (India) and may with great reason be suspected, in a certain degree, in *all*, we cannot hesitate to admit, *as an essential and valuable principle*, in the cure of fevers, *the introduction of mercury into the system, so as to affect the mouth in a moderate degree, with the view of removing obstructions, or other morbid affections of the liver*; of obtaining natural secretions, and of its *thus contributing, with the other means* that have been described, to a speedy and permanent cure²."

² Preface to a collection of treatises, &c. Dr. Balfour is well known in the literary and medical world, by his persevering enquiries into the nature of sol-lunar influence, upon the living body.

To the tracts, which I published, at this period, at Calcutta, were annexed reports of some of the cases of dysentery, which I had treated successfully with calomel and opium. The efficacy of this practice was then strongly inculcated; and a knowledge of it was soon diffused throughout India. I was, of course, not a little amused, a few days ago, in meeting with some observations, in a work published nineteen years afterwards, viz. in 1815, representing the use of calomel in dysentery, as a practice peculiar to the author, and some other navy surgeons, in the East Indies, who, he says, had employed it, "*without the least communication of sentiments on the subject, each conceiving his own plan to be perfectly unique*."

This admission is what I should have expected: and so much reliance have I on Dr. Balfour's candour, that I am persuaded, if he had had an opportunity of perusing the farther results of my researches, and of repeating my experiments, concerning the properties of mercury, he would have been equally ready to acknowledge its efficacy in the cure of fevers, without supposing it to act by removing obstructions of the liver, by purging, or salivation, or any other mode of evacuation, or by its specific properties, and without the smallest aid from emetics, cathartics, or bark. He would have been ready to allow, that it might produce all its curative effects, solely by its quality of an exciting power.

¹ The Influence of Tropical Climates, &c. by James Johnson, Esq. Surgeon, Royal Navy, London, 1815.

This was certainly a strange misconception. And it was no less strange that he should have been "surprised, long after this, (the period is not stated) to find that a German assistant surgeon made it a very common practice to cure dysenteries in this way." (p, 376.) This was not in reality very surprising ; since the German assistant surgeon, if he did not meet with these doctrines in India, might have seen them in Germany, as early as the year 1800.

In the course of 1797, and 1798, farther progress was made in this line of investigation. In confirmation of the efficiency of the mode of treatment, which I had introduced, in respect to acute diseases, reports of cases were communicated to me, by Dr. James Robertson, by Mr. Syme, surgeon of the Dublin East Indiaman, and by Mr. Crout, then surgeon of the Dover Castle, East Indiaman, and now of the St. Helena establishment. During the same period, and down to the year 1800, inclusively, I had also formed a collection of cases, taken from among a great many, that were treated, in various countries and climates, under my own care. These reports, amounting perhaps, in the whole, to materials sufficient for two moderate sized volumes, I have hitherto abstained from publishing, because experience had led me to believe, that, until some

considerable changes in the state of medicine should have taken place, innovations so complete, however true, were not very likely to experience a fair and candid examination. It is true, that, in this department, some essential changes have, in the intervening period, undoubtedly been operating. But whether they are yet of a nature, or degree, to render the publication of such a work expedient, in a view of public good, might still be questionable, were it not notorious, that the doctrines, which it embraces, have obtained such a degree of diffusion, without being proportionally understood, as to have rendered their constant and extending misapplication alarmingly detrimental to society. I am besides not without hopes, that the elucidations contained in these pages, may have the effect of ensuring a less unfavourable reception, to a work, on clinical medicine, conducted on the principles of inductive philosophy, and of rendering it more easy of comprehension, than at any preceding period. And it is also due to my friends, who have reposed unlimited confidence in me for that purpose, not to omit the first favourable opportunity, of giving to their opinions the chance of making their way in the world, under their own names.

For these, and other reasons, I have determined, in a separate work, shortly to give publi-

city to a collection of reports of cases, which have been treated by myself, and some of my friends, who have regarded medical doctrines under a similar point of view ; *not*, indeed, as *proofs of cures performed*, but as *examples of the application of principles in the treatment of diseases*. They will serve the purpose of a practical commentary upon the theories promulgated in these volumes ; in which, if it were necessary, I find it would be impossible to enter into adequate details.

As I never made, nor affected to make, a mystery, of any of my doctrines, in medicine, my still unpublished manuscripts, many of them above twenty years-old, have, at different periods, been shewn, to various medical men, in different countries, and without any injunction of secrecy. In this manner, their circulation has certainly been considerably increased ; unaccompanied, however, by any proper acknowledgment of their source : insomuch, that, in respect to medical doctrines, I have more than once, found myself in the situation of South, respecting one of his sermons : “ the ensuing discourse,” says he, “ lest I chance to be traduced for a plagiarist, by him who has played the thief, was one of those, that, by a worthy hand, was stolen from me.” Nor has the dexterity of the more experienced depredators, in this line, in altering the language, and pejorating the sense, of my doctrines, in order to render

them less unsuitable to the insipid taste of the times, been able to conceal their plagiarisms. If I had happened to perish, in the course of my investigation, in the Levant, it might have been, to the persons to whom I allude, in so far a convenient and advantageous incident; since, as it is not very probable that, in such case, any of the fruits of my researches, upon that occasion, would have found their way to the public, under my own name, it would, upon that subject, have left several original writers, instead of one, by preventing my making any troublesome reclamations.

As the connection between the first outlines of my doctrines, respecting the *cause* of epidemic diseases, published in India, and the first experiments, which were attempted to be made, in the Levant, concerning the Plague, has been rendered highly probable; so, that between my actual investigation of that malady, in 1815, and the publications which have since appeared, in such numbers in this country, upon the subject of epidemic diseases, might very readily be traced. But, to those, who have attentively perused the contents of these volumes, such disquisition would be superfluous; besides that it would require more space than I have now to spare.

Considering the almost inexplicable conduct, in respect to my doctrines, of the principal medical performers, upon the epidemic stage, in this

country, I trust it will appear, to every fair and candid mind, to be no more than a just assertion of my rights, whether attempted to be withheld or usurped, if I should enter into a detail somewhat circumstantial, respecting the manner of their circulation, in various countries ; a detail, which, under ordinary circumstances of injury and injustice, I am aware, might have, in some degree, the semblance of ostentation. Previous to my leaving India, in 1798, a copy of my tracts, as reprinted at Philadelphia, in 1797, was presented to me, by the surgeon of an American ship, that arrived in the port of Calcutta. In the New World, the doctrines they contained were considered of some importance ; and a long and candid review of them, was given, at the time, in the New York Medical Repository, and, doubtless, in other Trans-Atlantic Journals.

From America, my tracts were imported, in 1797, or 1798, into England ; and anathematised in the London Reviews. The medical faculty of Britain could scarcely have been more alarmed, if the plague itself had been introduced into the country, than they were at the appearance of these exotic tracts, these "*alieni libelli*" among them. The critics of the metropolis, fierce to strangers, like their painted ancestors of old¹,

¹ Britannos hospitibus feros.

were furious in their denunciations. "Such doctrines, and such practice," said these enlightened and liberal judges, "ought not to be *tolerated* any where." Having, upon my arrival in Britain, in 1799, chanced to glance my eye over two of these productions, which seemed to vie with each other in avowed hostility to experimental enquiry, I was curious, believing my tracts to have been reprinted in London, to purchase an English copy of this heterodox performance; and, for that purpose, repaired to the shop of Mr. Debrett, of Piccadilly, who was referred to, by the critics, as the publisher. At the very mention of the work, the poor bookseller's hair stood on end: *steteruntque comæ, et vox faucibus hæsit*. He denied being the publisher, as "had been falsely and maliciously represented by the reviewers," said he; acknowledged that a parcel of the work had, indeed, been sent to him, for sale, from America; but averred that he had not now a copy left, and, that, if he had, he would not sell it; for, he had been so abused by the faculty, ("mobbed," I think, was his expression) who threatened to withdraw their custom, and to do him all the injury in their power, if he should continue to sell it, that he found it necessary to promise to send back all the copies, he had not yet disposed of, across the Atlantic; which he had accordingly

done. Upon enquiring what fault the medical faculty found with it; whether they were pleased to consider it immoral, atheistical, or jacobinical, his reply was: "No, none of these; but they look upon it as extremely *whimsical*!"

That the application of the principles of inductive philosophy to the affairs of medicine might be regarded as new, I can readily comprehend: but upon what ground it could have been considered as whimsical, I am rather at a loss to divine. It was not thought necessary to repeat my experiments, (that would be to risk undermining the *principles* of the *conjectural* art of medicine!) in order to ascertain whether they were, or were not, uniformly followed by the results, which I had announced. Such processes would be inconvenient, and might be dangerous. It was both more easy, and more consistent, to assume, at once, that my conclusions were such as ought not to be received; and to pronounce excommunication accordingly. My modest critics, no doubt, pleased themselves with the expectation, that, by their sovereign fiat, enquiry and knowledge would stand still, and that they had only to hail delusion with an "*esto perpetuo*." But, notwithstanding the undistinguishing hostility, not simply of individual pseudo-critics, but of extensive and powerful combinations, my tracts have continued to circulate, and my doctrines to

be diffused, wherever medical discussion is known. Of their wide dissemination in America, some proof has been already given ; and, by a reference to the journals, as well as medical publications, of that country, it will be found, that my doctrines respecting non-contagion, in epidemic diseases, and the utility of mercury, in the treatment of acute maladies, begin almost universally to prevail.

As early as 1800, I met with an American criticism of my doctrines, in Germany, which belonged to Professor Reich, of Berlin. The same year, my “ Dissertation on the Source of Epidemic and Pestilential Diseases,” was republished, in the English language, in Hamburgh ; and, in 1805, in the German language, both at Leipsic, and at Cobourg, in Saxony. These are facts, which I know ; and it is reasonable to infer that there have been other criticisms, and impressions, with which I am unacquainted ; not to speak of the many thousand ways, in which the doctrines must have been spread, at second, third, fourth, tenth, and even twentieth hand, without reference or acknowledgement.

In London, independently of the importation from America, my Dissertation on the Source of Epidemic Diseases, as a separate tract, has, since the year 1800, formed an article of the medical circulating libraries. It was that year, let it also

be recollected, that the first attempt was made from England, (by Dr. Whyte) to investigate the plague, upon the avowed principle of non-contagion¹.

If any doubts could really exist, respecting their diffusion in England, they would be removed, by a reference to the discussions, which took place, a good many years ago, in London, when Dr. William Dick introduced into the British Capital, the administration of mercury, in acute diseases generally; which, to his honour be it said, he had been one of the first to adopt, and, by his experience and sagacity, to improve, and to extend, in the metropolis of India. From the merited confidence, which Dr. Dick had long enjoyed, among the British inhabitants of Asia, and the known efficacy of his treatment of diseases, he was enabled to succeed in his views, in defiance of adventitious obstacles of the most formidable and most unjustifiable kind.

¹ The reader will please to observe, that it is of no manner of consequence, how many persons may have been of opinion that *particular* epidemics have not depended on contagion; since no one has ever *proved*, or perhaps held, the general proposition, that *no epidemic disease can ever depend upon that source*. This is my precise doctrine, and, it must be evident, that as it is only by a general probation, applicable to all epidemics, that any valid proof can be given that each is not propagated by contagion; every attempt made, by other means, to prove this of particular epidemics, must be unavailing.

Thus, it is notorious, that, in Europe, as well as in Asia, and America, my doctrines, both respecting the cause, and cure of epidemic diseases, were widely disseminated, many years before discussions, respecting these maladies, had become fashionable in England ; and were, in fact, the cause of that change.

In the conduct of the recent authors of this country, upon epidemic diseases, and of those from whose official reports their books have been principally compiled, there are two points, besides a profound silence respecting the proper mode of employing mercury in fevers, on which they have observed a remarkable unanimity. They have all made the operation of this remedy a particular topic of discussion, in their observations on the fevers of Gibraltar and Spain ; and they have all done it the homage of invariably resorting to its aid, upon every occasion, in which they have been at a loss how to proceed, or in which their other means had all previously been tried, and failed ; *i. e.* in every case of disease, from which recovery would not have happened, under the usual action of the ordinary exciting powers, or of the surrounding elements ¹.

¹ For selections from the voluminous reports, which have been furnished upon this subject, by the medical officers of the navy and army, by the desire of their respective Boards, the curious may consult the works of Drs. Burnet, Pym, and

But, it is curious to observe, that, whilst the proper and only mode of operation of mercury, in the *cure* of diseases, should have been totally disregarded, these discussions should have been limited to two questions, which have absolutely nothing whatever to do with the matter; *viz.* whether it proved of the greatest service, when used as a purgative only, or given so as to produce ptyalism? (Burnet, p. 308.) These phænomena I have shewn to be mere indications of disease, or diminished excitement, the one of the alimentary canal, and the other of the salivary glands; and therefore always to be avoided. As these doctrines had, as has been shewn, been widely disseminated, it might have been expected, that *some*, at least, of the investigators of this branch of medicine, would have thought it right to repeat the experiments, by which such results were alledged to have been obtained, before they would have ventured to pass over *sub silentio*, or

Bancroft; and the discriminating reader will readily perceive how far these *selections* have been made subservient to the purposes of science, or of faction. We may congratulate the world on this newly-invented, cheap, easy, and expeditious manner, of manufacturing books *ad libitum*. Every succeeding epidemic may be expected to produce a fertile harvest of volumes; and should scientific truths not be developed in abundance, it will not be for want of testimony!

openly to reject the important conclusions, which had been confidently, and not vaguely, deduced from them.

Did any of them, then, give to this remedy, *in this manner*, a fair trial? If so, they must have found it either to succeed, or to fail. But neither of these results have been allowed to transpire; although there could have been no motive at least for concealing the latter. And, it cannot, in any case, be supposed, that individual medical officers could, for a purpose, have predetermined not to find the laws of nature uniform, as they relate to this subject. Many of them were, no doubt, *bona fide*, in the predicament, in which Dr. Bancroft finds it expedient to represent himself, of not recollecting “ever to have read” my doctrines¹. It is scarcely to be presumed, however, that they could have all been endowed with such convenient memories, or such an indifference to solid research. But it is true, that, as none could expect preferment, some might dread ridicule, and others censure, for adopting professional improvements, which had not emanated from persons having promotion in their

¹ This writer’s presumptuous pretensions, and wilful misrepresentations, set forth since the publication of my first volume, in “*A Sequel*” of 500 pages, to his “*Essay*” of 800 pages, on Yellow Fever, I shall take occasion to notice as they appear to merit.

gift. And it is certain, that, neither the interests, nor the reputation, of medical candidates, (*des aspirans*) could be injured, by not choosing to be amongst the foremost to adopt innovations, not recommended from authority, however obviously useful. In such cases, there can be no possible danger, whether the defect be real, or feigned, in being obstinately blind.

Sir James Fellowes observes, (Reports, p. 406.) “ *I never saw any advantage from the large quantities of mercury, recommended to be employed in this disorder.*” This is, however, very meagre information. Sir James ought to have informed us more specifically, *whether he did see large quantities applied, and in a proper manner, without being followed by any advantage.* This is, I believe, what Sir James could not have seen, unless he were gifted with the capacity of seeing what cannot happen. Neither could he ever have seen sufficiently large quantities of mercury applied, *in a proper manner*, with merely *no advantage* as a result. A remedy, not of a negative nature, being administered in adequately large quantities, and in a proper manner, *must*, if it be also of the appropriate kind, produce beneficial effects ; and, if it be not, it *must* produce *injurious ones*. Why, then, did not Sir James Fellowes also inform us of those latter effects ? The fact appears to be, that Sir James

did never see this remedy administered in adequate quantities, or in a proper manner, in the fevers of Spain : otherwise, it is to be presumed, that, as a man of veracity, he would have felt it necessary to have spoken very differently of its results. These observations will equally apply to all the other writers in this line. With surprising harmony, they appear to be all attuned to the same key : “ *cantilenam eandem canunt ;*” and I have only singled out Sir James Fellowes’s text for my commentary, because it happened to be the first apt example that I found at hand.

On this subject, I do not choose at present to say more. Nor is it probable, unless a perseverance in the same extraordinary line of conduct should call for farther animadversion, that I shall ever feel inclined to recur to so disagreeable a topic. To this reserve, however, I am prompted by motives very different from apprehension of any species of logic, which it is in the power of the enemies of medical truth and justice *openly* so employ : and if I should ever judge it expedient to depart from it, the persons, to whom I allude, will find, that this is scarcely an opening of the exposition, which is at my command. These are matters, however, entirely for their own consideration. It can be of not the smallest consequence to me, whether they may prefer losing all their patients, who would not have re-

covered without any medical treatment, *secundum artem conjecturalem medicinæ*, to saving all who are not, from organic lesion, already incurable, by the due application of ascertained principles. It can, at no time, be an object of my ambition to be quoted, as an authority, by persons, who are either incapable of distinguishing between the validity of tradition and testimony, and induction of experiment, in proof of scientific conclusions ; or have so depraved a taste as to prefer that which is most inappropriate. But it is of very considerable consequence, that, through the medium of public discussion, those medical practitioners, who, from whatever motives, have a reluctance to adopt or to acknowledge them, should be compelled to apply efficient methods of treatment, even if new, and not emanating from themselves, for the preservation of human lives. This object shall be the principal guide of my future, as it has been of my past, proceedings.

From all that has been said, it follows, that it is not by its ideal virtues, as a purgative, alterative, deobstruent, sudorific, diuretic, cholagogue, sialagogue, or specific, that mercury cures fevers, and other acute diseases ; but solely by its operation as an exciting power. When it removes the original disease, by the intervention of ptyalism, or purging, this is but the substitution of one

disease for another ; and, as the disease substituted, as denoted by these phænomena, is slight, it generally soon ceases spontaneously. Under these circumstances, the benefits produced, such as they are, do not arise in consequence, but in defiance of the symptoms of purging and salivation. But the beneficial results of the proper administration of mercury, as an exciting power, in severe, as well as in inferior degrees of disease, wherein it is appropriate, are decisive, rapid, and permanent¹. They are always to be relied upon, and confidently to be predicted. There are here no indications, and contra-indications, beating each other's brains out. It will produce similar, and determinate effects, in *all* cases of the same disease, that are not, from organic lesion, already incurable.

It may be necessary to observe, that, the treatment, which is here described, has nothing in common, either in principles or practice, with that, which was formerly used, in India, in cases of hepatitis. That treatment, as described, in a letter from Mr. Paisley, head surgeon at Madras, inserted in Mr. Curtis's "Diseases of India," by Dr. Girdlestone, in his "Treatise on Hepatitis," and by others, consisted of the misapplication of mercury, by its administration, in inadequate doses, night and morning only, with the same deleterious effects which accompanied the employment of

that remedy, for the last three centuries, in syphilis. Administered according to principle, it is capable of rapidly curing all the diseases, in which it is an appropriate remedy, that are not, from extensive lesion of organs, already incurable, without admitting the substitution of the diseases, which, under its misapplication, or the ordinary mode of employing it, never fail to arise, from the diminution, suspension, or cessation of its own action.

The manner, in which both health and disease succeed the application of this, and other exciting powers, called remedies, will be fully explained in my "Elements of Medicine."

THE END.

Tables, and other Illustrations, which I had proposed to annex to this volume, not being yet completed, they will, together with the Errata and Index to the two volumes, be shortly transmitted to the publisher.

